SOCIAL POWER AND RESPONSIBLE BEHAVIOR

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SUMMARY

According to conventional wisdom, a good leader is a responsible leader, i.e., someone who is disciplined, self-possessed, persistent, focused on strategic long-term goals and respectful of the interests of others. But does the experience of being in charge and controlling the resources of others affect the capability and motivation to behave responsibly? The current dissertation addresses this question in investigating the short-term effects of social power on self-control (as opposed to questions such as whether trait self-control facilitates the acquisition of power which are not studied here).

The state of research did not allow drawing a clear conclusion on the nature of the relationship between power and self-control. With regard to theory, the two major power theories make conflicting predictions. The social distance theory (Magee & Smith, 2013) postulates that power should improve self-control performance, because powerholders are thought to engage in higher-level construal, which has been proven to benefit self-control. In contrast, the approach/inhibition theory (Keltner, Gruenfeld, & Anderson, 2003) suggests that power should worsen self-control performance as a consequence of the powerful's heightened attention to rewards and drive to experience these rewards immediately. Empirical findings are similarly contradictory. On the one hand, participants in high-power conditions are better able to focus on the task at hand (Guinote, 2007b; Smith, Jostmann, Galinsky, & van Dijk, 2008), and they persist longer and make more attempts to solve (unsolvable) tasks (Guinote, 2007a) than participants in low-power conditions. On the other hand, they are worse at suppressing thoughts (Guinote, 2007c), withstanding impulses to act (Scholl & Sassenberg, 2015), and they take more risks (Anderson & Galinsky, 2006).

This dissertation aims to resolve the contradiction by considering self-control to be a two-dimensional construct whose initiatory and inhibitory components are differentially affected by power. More precisely, it is hypothesized that social power would benefit task performance if the task requires initiatory (start) self-control but would impair task performance if the task requires inhibitory (stop) self-control. However, a first direct replication as well as a conceptual replication conducted in the laboratory with predominantly student participants found no effect of the between-person power manipulations on various self-control *performance* measures.

To explore the possibility that these null findings resulted from lacking personal relevance of power and self-control in the laboratory, an experiment and a quasi-experiment investigated the relationship between these two constructs with non-student samples in everyday life where having power and exerting self-control are actually consequential. Contrary to prevalent research practice to examine the effects of differences in power between-person, studying power in the real world implies that power also varies to a substantial extent in rather short time spans within-person. Given the limited scientific

knowledge about within-person manipulations of power, a pilot laboratory experiment was conducted which showed that (psychological) power can indeed be manipulated within-person by means of a role-play.

The aforementioned experiment and quasi-experiment tested a theoretical framework that links within-person variations in objective power and psychological power to start, stop (and interpersonal) self-control in organizational settings. Key predictions of the theoretical framework were supported: Variation in objectively demonstrable control of valued resources (i.e., situational power) was accompanied by variation in subjective power, such that participants experiencing high situational power also reported a high sense of power, high personal power and a high sense of responsibility.

In both studies, real-world power affected real-world self-control *motivation*. However, the effects were complex: From a between-person perspective on both studies, participants in low power situations (i.e., situational power) reported on a descriptive level both more stop (supporting our hypothesis) and also more start self-control (contradicting our hypothesis) than participants in high power situations. From a within-person perspective, the effects were inconsistent across the two studies: In the experiment, personal power was negatively related to both start and stop self-control, whereas in the quasi-experiment personal power was positively related to start self-control but unrelated to stop self-control. This divergence might have resulted from differences in the method of the two studies.

Altogether, these inconsistent results emphasize the necessity to complement between-person laboratory experiments by the investigation of experiential episodes of varying degrees of objective and subjective power. The findings contribute to a more fine-grained understanding of the relationship between social power and self-control as well as of the way power dynamics operate in real-world organizational settings. Future research might benefit from the contextual approach to study power as presented in this dissertation to investigate outcomes other than self-control.

CHAPTER 1: General Introduction

« *Une grande responsabilité est la suite inséparable d'un grand pouvoir.* »

Voltaire (Writer, historian and philosopher, 1694 - 1778)

This quotation by Voltaire (prominently cited by the fictional character Benjamin "Ben" Parker, uncle of Peter Parker alias Spiderman) puts it in a nutshell: By definition, powerful people such as political leaders or leaders in organizations (i.e., individuals who control the material and immaterial resources of others, cf. Galinsky, Gruenfeld, & Magee, 2003; Keltner et al., 2003) have responsibility. However – does power always lead to more responsible behavior? Apparently not, or Voltaire and others throughout history would not have seen the need to remind their fellow human beings of this truth.

Responsibility is a concept with multiple meanings that has been studied from the perspective of law, ethics/philosophy, economy and psychology (Nordbye & Teigen, 2014; Witt & Stahl, 2016). Responsibility can be understood as causing certain consequences through action or inaction (causality), the capability for rational conduct (a mental capacity), foreseeing and intending certain consequences (a mental state), a duty arising from social roles, and a condition of being answerable to others (Schlenker, Britt, Pennington, Murphy, & Doherty, 1994). The latter two meanings are related to the concept of accountability, which is defined as "implicit or explicit expectation that one's decisions or actions will be subject to evaluation by some salient audience(s) (including oneself), with the belief in the potential for either rewards or sanctions based on these evaluations" (Hall, Royle, Brymer, Perreweé, Ferris, & Hochwarter, 2006, p. 88).

Within leadership literature, the idea of responsible leadership has received much attention (e.g., Cameron, 2011; Voegtlin, Patzer, & Scherer, 2012). In these discussions, responsible leadership has been defined, for instance, in the following ways:

> "Responsible leadership can be understood as the art of building and sustaining social and moral relationships between business leaders and different stakeholders (followers), based on a sense of justice, a sense of recognition, a sense of care, and a sense of accountability for a wide range of economic, ecological, social, political, and human responsibilities'' (Pless, 2007, p. 451).

> "Responsible leadership can thus be understood as the awareness and consideration of the consequences of one's actions for all stakeholders, as well as the exertion of influence by enabling the involvement of the affected stakeholders and by engaging in an active stakeholder dialogue. Therein responsible leaders strive to weigh and balance the interests of the forwarded claims" (Voegtlin, 2011, p. 59).

These definitions share the notion that responsible leaders respect the interests of others and take the consequences their actions have (for others) into consideration. Additionally, they imply the responsible leaders' engagement in long-term thinking. In this dissertation, I define responsible behavior as disciplined, considerate, and deliberate behavior in line with the definitions cited above, and mainly operationalize it by the established psychological construct of (interpersonal) self-control (e.g., Tangney, Baumeister, & Boone, 2004; Vohs, Lasaleta, & Fennis, 2009). As leadership can be considered a way to exert power in a specific (organizational) context, I will focus on the more basic construct of power in this dissertation (cf. Williams, 2014).

The relationship between power and self-control can be investigated from two perspectives. First, one could ask whether self-control facilitates the acquisition of power (McIntyre, von Hippel, & Barlow, 2016). On the one hand, individuals following their desires and concentrating on their own benefit might be perceived as having more control, more influence, and being less dependent – in short more powerful – than targets who behave in a self-controlled and prosocial manner, as observers infer that these individuals experience less constraint and are able to act in line with their own volition to a greater extent (Magee, 2009). On the other hand, individuals high in self-control and low in selfishness could acquire power positions more easily, because high self-control is associated with better grades and better interpersonal skills (de Ridder, Lensvelt-Mulders, Finkenauer, Stok, & Baumeister, 2012; Tangney et al., 2004), and individuals who consider others' needs are granted positions higher up in hierarchy (Flynn, 2003; Flynn, Reagans, Amanatullah, & Ames, 2006; Hardy & Van Vugt, 2006; Willer, 2009).

Second, one could ask how power affects self-control. One the one hand, the powerful might *not* behave in a self-controlled way because they tend to experience fewer constraints and greater freedom to follow their desires. On the other hand, having power could also result in self-controlled behavior because thinking ahead and being disciplined might help secure or strengthen one's power position.

The current dissertation aims to investigate the short-term effects of power on self-controlled behavior and is not interested in the long-term effects of personality on the achievement of power positions (or the other way around). I want to answer the research question "What is the effect of power on self-control?" by presenting one quasi-experimental and four experimental studies. I extend previous power research by not only conducting laboratory research as is common within power literature, but by also studying organizational contexts.

This introductory chapter is made up of three parts. In the first part I discuss definitions of power, self-control, and related constructs that need to be differentiated. In the second part I present a summary of research on social power with a focus on power's effects on self-

control and self-regulation. The second part closes with a discussion of the deficits of social power research the current dissertation seeks to address. Finally, the third part of this chapter introduces the current research and provides an overview on the following chapters.

Definitions

Both power and self-control are broad constructs. Various definition attempts presented in the respective literatures show partial overlap but also stress different aspects. This suggests that both constructs are multidimensional. In addition, one encounters several related concepts in both literatures that need to be distinguished from the two focal constructs.

Power

The literature differentiates social power from personal power. Personal power is the extent to which power holders are capable of acting with agency or of producing their intended effects in the environment (Overbeck & Park, 2001; van Dijke & Poppe, 2006) as well as "power over oneself and freedom from the influence of others" (Lammers, Stoker, & Stapel, 2009, p. 1544). In contrast, social power is derived from one's relationships to others (Fiske, 1993; Wolfe & McGinn, 2005) meaning that one cannot have social power in the absence of other people. Power is a truly relational construct in this view.

Social psychology's approach to define social power can be classified into two primary domains. The first position considers power to be the (potential) ability to *influence* others' behavior (Dahl, 1957; Emerson, 1962; French & Raven, 1959). However, Fiske and Dépret (1996) point to a problem of equating power with (potential) influence. In the power-as-influence view, power is a consequence of social interactions rather than a structural characteristic and thus cannot be manipulated as an independent variable. Another problem with this definition might be that it confounds the power of the agent with the weakness of the target (Fiske & Berdahl, 2007). Hence, it is now more common to define power in terms of *control over the outcomes* for someone else (e.g., Andersen & Berdahl, 2002; Fiske & Dépret, 1996; Galinsky et al., 2003; Keltner et al., 2003; Thibaut & Kelley, 1959).

Power-Related Concepts

When studying social power, several other related notions – status, authority, dominance, and leadership – require clarification. In a broad sense, *status* (as the outcome of an evaluation of attributes) produces differences in respect and prominence (Keltner et al., 2003); more narrowly defined, status can stem from the power an individual possesses due to social group membership (Schmid Mast, 2010). It is possible to have status without power (e.g., Mother Teresa) and power without status (e.g., a corrupt politician; for an extensive comparison of power and status see Fragale, Overbeck, & Neale, 2011). *Authority* can be

defined as power derived from institutionalized roles (e.g., supervisor, site manager), which is alternatively called position power or structural power (Schmid Mast, 2010). *Dominance* covers personality dominance and dominance behavior. The first can be understood as an individual difference variable describing the extent to which an individual has the desire to influence or control others (Schmid Mast, 2010); the latter is any behavior used with the intention to gain or maintain power over other people (Keltner et al., 2003).

Leadership involves influencing individuals to willingly contribute to the shared goal of a group and coordinating their actions in order to reach a common goal (Kaiser, Hogan, & Craig, 2008). Some authors think that power and leadership must be clearly differentiated (e.g., Galinsky, Jordan, & Sivanathan, 2008; Wisse & Rus, 2012). However, power can also be seen as an essential leadership dimension allowing people who exhibit leader behaviors to be viewed as effective leaders (Hollander & Offermann, 1990) as well as a *process* through which leaders direct followers' actions towards a common goal (Giessner & Schubert, 2007; Voyer & McIntosh, 2013). Although studied within different research areas, power and leadership show conceptual overlap (Williams, 2014).

Self-Control-Related Concepts

When interested in the notion of "self-control" from a psychological perspective, one is confronted with various terms and definitions, such as self-regulation, impulsivity, executive functions, willpower, and effortful control, among others. Part of this diversity might stem from the fact that the idea of "self-control" has been investigated within social and personality psychology (e.g., Ent, Baumeister, & Tice, 2015; Friese & Hofmann, 2009), behavioral economics (e.g., Halali, Bereby-Meyer, & Ockenfels, 2013; Joireman, Sprott, Spangenberg, 2005; Rachlin & Jones, 2008), cognitive psychology (e.g., Bari & Robbins, 2013; Hofmann, Schmeichel, & Baddeley, 2012), clinical psychology (e.g., Crean, de Wit, & Richards, 2000), developmental psychology (e.g., Forstmeier, Drobetz, & Maercker, 2011; Mischel & Mischel, 1987; Moffitt et al., 2011), and criminal psychology (e.g., Gottfredson & Hirschi, 1990). The terms listed above are sometimes used interchangeably (Duckworth & Kern, 2011). However, some authors make an effort to distinguish (some of) these concepts.

Self-regulation refers to the general process by which people adopt and manage various goals and standards for their thoughts, feelings, and behavior, and then ensure that these goals and standards are met (e.g., Carver & Scheier, 1982, 1990). Accordingly, self-regulation incorporates three main components, namely (1) standards such as ideals, values, morals, social expectations, and long-term goals that individuals strive to attain, (2) the motivation to invest effort in reducing discrepancies between standards and actual states and (3) sufficient capacity to reduce the discrepancy (despite being confronted with obstacles along the way; Fujita, 2011). Self-regulation involves numerous challenges, such as choosing a goal, planning how to pursue this goal, implementing these plans, and shielding

goals from competing concerns (e.g., Gollwitzer, 1990; Karoly 1993).

Advocates of a distinction between self-control and self-regulation proposed to define *self-control* as either of the two following sub-components of self-regulation. On the one hand, self-control has been described in terms of inhibition (e.g., Alberts, Martijn, Greb, Merckelbach, & Vries, 2007; Burkley, 2008; Martijn, Tenbült, Merckelbach, Dreezens, & de Vries, 2002; Muraven, Tice, & Baumeister, 1998), for instance as "the overriding or inhibiting of automatic, habitual, or innate behaviors, urges, emotions, or desires that would otherwise interfere with goal-directed behavior" (Muraven & Slessareva, 2003, p. 894), or "self-regulatory processes [...] that aim to override unwanted, prepotent impulses or urges (such as the urge to indulge in a high-calorie desert when on a diet)" (Hofmann et al., 2012). On the other hand, self-control has been characterized as the promotion of one's abstract and distal goals when they are threatened by competing concrete and proximal goals (e.g., Ainslie, 1975; Fujita, 2011; Hoch & Loewenstein, 1991; Kirby & Herrnstein, 1995; Mischel, 1974; Mischel, Shoda, & Rodriguez, 1989; Rachlin, 1995; Rachlin & Green, 1972; Thaler & Shefrin, 1981). The proponents of self-control as goal-conflict criticize that the definition of self-control as effortful inhibition conflates one strategy that people might use to promote a particular end with the end itself (Fujita, 2011). They point to the possibility of attaining this end with alternative strategies such as regulating the availability and opportunity to indulge in temptations or the re-construal of temptations.

Impulsivity has been defined as an inability to inhibit inappropriate behaviors, a preference for a small immediate reward versus a larger but delayed one, an inclination to act without forethought or before all necessary information is available, a tendency to act on a whim and, in so doing, disregard a more rational long-term strategy for success, novelty/sensation-seeking, and an increased propensity to engage in risky behaviors (Bari & Robbins, 2013; Madden & Johnson, 2010; Reynolds, Ortengren, Richards, & de Wit, 2006). It can be regarded as a consequence of impaired executive functioning (Bari & Robbins, 2013). Impulsivity could therefore be considered the antonym of self-control.

Executive function has been defined in many ways, for instance very broadly as functions associated with the frontal cortices, whereas others define executive function by identifying its components (Bickel, Jarmolowicz, Mueller, Gatchalian, & McClure, 2012). According to an influential taxonomy (Miyake, Friedman, Emerson, Witzki, Howerter, & Wager, 2000) these are: (1) updating, which refers to working memory operations such as keeping information in an active, quickly retrievable state and shielding this information from distraction, (2) inhibition, which refers to the ability to inhibit prepotent impulses when necessary, and (3) shifting, which refers to the ability to switch back and forth between multiple tasks or mental sets. Since an individual's executive function system plays an important role in the consideration of and the planning for the individual's future (Bickel et al.,

2012), executive function is related to the concepts of self-regulation and self-control.

Self-Control

The most basic distinction within self-control is the distinction in trait and state self-control. Studies about *trait self-control* as the stable individual ability to align behavior with standards or long-term goals have investigated self-control predominantly as an independent variable (for exceptions see Diamond, Briand, Fossella, & Gehlbach, 2004; Perrone, Sullivan, Pratt, & Margaryan, 2004): Trait self-control is positively linked to success in school and at work, health, successful interpersonal functioning, as well as well-being, adjustment (de Ridder et al., 2012; Moffitt et al., 2011; Tangney et al., 2004), and happiness (Cheung, Gillebaart, Kroese, & de Ridder, 2014; Hofmann, Luhmann, Fischer, Vohs, & Baumeister, 2013). It is negatively related to unemployment and substance abuse (Daly, Delaney, Egan, & Baumeister, 2015; Moffitt, Poulton, & Caspi, 2013). In addition, self-control has been investigated as *state self-control*, which may change from one situation to the next. State self-control has had the role of a dependent variable; it is also treated as such in this dissertation.

Apart from the generally accepted distinction between trait and state self-control, there is a lively debate on the “real” nature of state self-control: is it a limited, physiologically-based resource or a motivational construct? An overview on this debate is given in the following paragraphs.

The strength model of self-control (Baumeister, Vohs, & Tice, 2007; Muraven & Baumeister, 2000) claims the existence of a limited mental resource that has the capacity to (effortfully) alter the self’s behavior, cognitions, and emotions. In formulating their theory, Baumeister and colleagues were inspired by the observation that exerting self-control reduces self-control performance in subsequent self-control situations (cf. dual task paradigm), a phenomenon known as ego-depletion. The limited mental resource is often compared to a muscle that gets fatigued with heavy use but can recover after a break. Although one meta-analysis found support for the ego-depletion effect and strength model hypotheses (Hagger, Wood, Stiff, & Chatzisarantis, 2010), other meta-analyses questioned the existence (Carter, Kofler, Forster, & McCullough, 2015, Carter & McCullough, 2014) or at least the magnitude (Hagger et al., 2016) of the ego-depletion effect.

Apart from the question of whether ego-depletion is a real phenomenon, the idea of self-control as a limited resource was challenged on multiple grounds. To give two examples: First, the limited mental resource is still elusive. It has never been observed or measured directly, but it has been inferred from patterns of performance results in subsequent tasks (Inzlicht, Schmeichel, & Macrae, 2014). One idea to more directly measure self-control expenditure was based on the assumption that self-control exertion would cause blood glucose levels to drop as the brain would consume more glucose to tackle self-control

challenges than to execute other mental processes (Gailliot & Baumeister, 2007). However, this idea has not been successful because findings apparently supporting this idea either could not be replicated or could be explained differently (e.g., Molden, Hui, Scholer, Meier, Noreen, D'Agostino, & Martin, 2012).

Second, if state self-control performance were determined only by the limited mental resource, counteracting ego-depletion by manipulating an individual's psychology should not be possible. For instance, studies have shown that increased task motivation (Muraven & Slessareva, 2003), perceptions of (rather than actual) mental effort (Clarkson, Hirt, Jia, & Alexander, 2010), beliefs that self-control is unlimited (Job, Dweck, & Walton, 2010), perceptions of a task as autonomous versus controlled (Moller, Deci, & Ryan, 2006), and perceived self-sufficiency (Boucher & Kofos, 2012) eliminate ego depletion. Other intervention studies have demonstrated that affirming some core value (Schmeichel, & Vohs, 2009), smoking cigarettes (Heckman, Ditre, & Brandon, 2012), watching a favorite television program (Derrick, 2013), or even praying (Friese, & Wänke, 2014) also counteract ego-depletion. All these findings are difficult to reconcile with the limited resource idea (for more objections against the resource-based view of self-control see Inzlicht et al., 2014; Masicampo, Martin, & Anderson, 2014).

Building on these arguments, Inzlicht and Schmeichel (2012) presented the process model of ego-depletion. According to this model, self-control exertion at Time 1 is followed by a shift in motivational orientation and a shift in attentional focus that together undermine self-control performance at Time 2. More precisely, the shift in motivational orientation indicates that people are less motivated to engage in further effortful control after an initial self-control expenditure and are more inclined to do things that they see as more personally gratifying. This motivational process is accompanied by a shift in attention toward cues signaling rewards. In this view, ego-depletion is the result of shifts in motivation, attention and emotion.

A more inclusive state self-control theory was put forward by Kotabe and Hofmann (2015). In their integrative self-control theory, self-control is conceptualized as a process with the three main components of activation, exertion, and enactment. The core element of their self-control conceptualization is an intrapsychic conflict between "desire" and a higher order goal. Within the activation phase, desire and an at least partly incompatible higher order goal generate desire-goal conflict. This conflict marks the transition to the exertion phase in that it activates control motivation. Control motivation and control capacity interactively determine potential control effort. In the enactment phase, the intended behavioral outcome of the previous phase is realized if there are no enactment constraints.

Taken together, it seems fair to assume that state self-control is a function of attention, motivation, and ability (i.e., all the potential non-motivational cognitive resources an individual can draw on to face the desire-goal conflict such as directed attention, inhibitory capacity, cf.

executive function). The current dissertation is based on this conceptualization of self-control. I investigate potential effects of power on attentional and motivational aspects of self-control, but do not assume that power alters self-control ability.

How Social Power Impacts Behavior

Having – or lacking – power has been shown to influence how people perceive, how they think, and how they act (for reviews see Galinsky, Chou, Halevy, & Van Kleef, 2012; Galinsky, Rucker, & Magee, 2015; Guinote, 2017). Thus, in the words of Keltner and colleagues (2003), power is "a basic force in social relationships" (p. 265). In the following, I first present the most influential theoretical developments proposed in the last two decades, the approach/inhibition theory (Keltner et al., 2003) and the social distance theory (Magee & Smith, 2013). I then provide an overview on empirical findings relevant to this dissertation, namely self-regulation/self-control and interpersonal behavior.

Theoretical Perspectives

The approach/inhibition theory (Keltner et al., 2003) is an intrapersonal and motivational account of the effects of power. Power is thought to stem from interacting variables on the individual (e.g., extraversion, dominance, height, prominent jaw), dyadic (e.g., investment in and commitment to the relationship), within-group (e.g., status, authority), and between-group level (e.g., socio-economic status, majority or minority group affiliation). Keltner and colleagues, based on Gray's (1991) original reinforcement sensitivity theory (RST), suggest that (1) high power activates the behavioral approach system (BAS) which is sensitive to rewards and opportunities, and (2) low power activates the behavioral inhibition system (BIS) which is sensitive to punishment, threat, and uncertainty. Accordingly, Keltner and colleagues hypothesize that high power triggers approach-related positive affect, attention to rewards, automatic cognition, and disinhibited behavior, whereas reduced power activates inhibition-related negative affect, systematic cognition, and situationally constrained behavior.[1]

In contrast, the social distance theory (Magee & Smith, 2013) posits an interpersonal and cognitive account to explain power's effects. It relies on the assumption that differences in control over valued resources cause differences in dependence and is built on two basic propositions. The first proposition is that asymmetric dependence between two individuals gives rise to asymmetric experiences of social distance, i.e., the subjective perception or experience of distance from others, with the high-power individual feeling more subjective distance than the low-power individual. Magee and Smith argue that the asymmetric social

[1] Two attempts to refine or extend the approach/inhibition theory have been published (Anicich & Hirsh, 2017; Hirsh, Galinsky, & Zhong, 2011), at least partly because the underlying RST has undergone revision with the key change of partial reformulation of the tasks of the three motivational systems - the BIS, the BAS, and the Fight-Flight-Freeze System (FFFS) (Gray & McNaughton, 2000; McNaughton & Gray, 2002). However, they are not presented here as the details that were changed are not relevant to the current dissertation.

distance results from a combination of an individual's motivation for affiliation and his/her expectations of affiliation from the other party. Because of their outcome dependency, low-power individuals are motivated to affiliate while expecting only little affiliation within the relationship, whereas high-power individuals, who are less dependent on their low-power counterparts, have less motivation to affiliate with their low-power counterparts (Lammers, Galinsky, Gordijn, & Otten, 2012).

The second principle, inspired by construal level theory (Trope & Liberman, 2010), assumes that because high-power individuals have a greater sense of social distance they engage in more abstract mental representation (i.e., higher level construal) than low-power individuals. Consequently, Magee and Smith suggest that powerholders (a) feel greater subjective certainty, (b) behave more in line with their values, (c) select and pursue goals more efficiently, (e) show better self-control, (f) use categorical abstractions of targets' behavior, and (g) perceive other people more instrumentally.

In summary, the approach/inhibition theory and the social distance theory agree on the predictions that power makes people "asocial" (i.e., disinterest in others' mental states, inaccuracy in mental state inference, stereotyping, instrumental person perception, imperviousness to social influence), more confident, more authentic, and more effective in goal pursuit. However, they make different predictions regarding (1) the relative influence of desirability versus feasibility on decision-making and (2) self-control. First, whereas the approach/inhibition theory makes a valence-based prediction in assuming that the powerful would pay attention to positive rather than negative information more selectively in both the desirability and the feasibility domains, the social distance theory suggests that the powerful will be influenced more by desirability relative to feasibility concerns, as abstract thinking is closely linked to desirability. Second, the approach/inhibition theory suggests that power impairs self-control due to the powerfuls' heightened attention to rewards and their drive to experience these rewards immediately, whereas the social distance theory predicts that power is positively associated with self-control due to the powerfuls' use of high-level construal of goals and situations.

Social Power Affects Self-Regulation

Self-regulation entails pursuing a goal, knowing when to act, and managing self-control challenges. Several key findings have emerged with regard to goal pursuit. First, research indicates that power leads to more behavior in line with own salient goals and less goal contagion (Gruenfeld, Inesi, Magee, & Galinsky, 2008, Jia, Koh, & Tan, 2018; Karremans & Smith, 2010; Laurin et al. 2016; Overbeck & Park, 2006). Second, power is tied to a greater readiness to act in the service of goal pursuit (Guinote, 2007b; Scholl & Sassenberg, 2014b, 2015). Third, power allows focusing on the pursuit of specific goals the powerholder has committed to. For instance, the powerful experience less goal conflict in settings with multiple

goals (Cai & Guinote, 2017; Schmid, 2018) and are better able to focus on what is relevant (Guinote & Ong, 2012; Schmid, Schmid Mast, & Mast, 2015; Slabu & Guinote, 2010; Whitson, Liljenquist, Galinsky, Magee, Gruenfeld, & Cadena, 2013).

Having power also relates to the initiation of (approach) behavior (Galinsky et al., 2003; Lammers, Stoker, & Stapel, 2010; Maner, Kaschak, & Jones, 2010; Smith & Bargh, 2008). This has even been demonstrated on the level of physiological processes (Boksem, Smolders, & De Cremer, 2012; Scheepers, de Wit, Ellemers, & Sassenberg, 2012). In a similar vein, numerous studies have provided evidence that the powerful tend to take risks more willingly than their powerless counterparts (Anderson & Galinsky, 2006; Carney, Cuddy, Yap, 2010; Fast, Gruenfeld, Sivanathan, & Galinsky, 2009; Fast, Sivanathan, Mayer, & Galinsky, 2012; Hiemer & Abele, 2012; Jordan, Sivanathan, & Galinsky, 2011; Maner, Gailliot, Butz, & Peruche, 2007; Ronay & Von Hippel, 2010; but see Ranehill, Dreber, Johannesson, Leiberg, Sul, & Weber, 2015). This tendency seems to be especially pronounced when the position of power is unstable (Jordan et al., 2011), but this effect might be reversed for dispositionally power-motivated individuals (Maner et al., 2007). The explanatory mechanism for this risk-taking propensity of the powerful seems to be (over-) confidence (Fast et al., 2009; Fast et al., 2012).

Social Power Affects Interpersonal Behavior

The approach/inhibition theory and the social distance theory agree on the default assumption that powerful people are less sensitive in interactions with other people, i.e., are able to sense, accurately perceive, and appropriately respond to their interpersonal and social environment to a lesser degree (Bernieri, 2001) than their powerless counterparts. On the one hand, previous studies documented in support of this theoretical claim that power is related to disinterest in others' mental states (Galinsky, Magee, Inesi, & Gruenfeld, 2010; Blader, & Chen, 2012; van Kleef, Oveis, van der Löwe, Luo Kogan, Goetz, & Keltner, 2008; Woltin, Corneille, Yzerbyt, & Förster, 2011, Experiment 3), empathic inaccuracy (Georgesen & Harris, 2000; Gonzaga, Keltner, & Ward, 2008; Neuberg & Fiske, 1987), and selfish behavior (Bendahan, Zehnder, Pralong, & Antonakis, 2015; de Cremer & van Dijk, 2005; Lammers, Stapel, & Galinsky, 2010; Wiltermuth & Flynn, 2013).

On the other hand, power has been shown to increase interpersonal sensitivity and attention (Overbeck & Park, 2001, 2006; Schmid Mast, Jonas, & Hall, 2009) as well as prosocial behavior and generosity (Côté et al., 2011; DeCelles, DeRue, Margolis, & Ceranic, 2012; Tost, Wade-Benzoni, & Johnson, 2015). This contradiction has been explained by the strong goal-dependency of the behavior of high power individuals. That is, if their personality or the situation advise them to do so, the powerful can deviate from the default and behave responsively and in a prosocial manner instead.

The Predominant Methodological Approach and its Critique

In addition to the content-related question concerning the nature of the relationship between power and self-control, this dissertation seeks to address methodological and theoretical issues within previous power research.

From a methodological point of view, most of the studies cited in this introduction share the following characteristics: They use relatively small student samples and employ a one-shot, between-person, experimental two-cell design, comparing high power conditions to low power or control conditions. Typically, manipulations without a social-interactional element are intended to create differences in (psychological) power, which should then result in differences in the dependent variable. These manipulations include *experiential priming* in the form of recall of past powerful or powerless experiences (e.g., Duguid & Goncalo, 2012; Fast et al., 2012; Galinsky et al., 2006; Guinote, Weick, & Cai, 2012; Smith & Trope, 2006), *semantic priming* (e.g., Johnson & Lammers, 2012; Smith & Bargh, 2008; Smith et al., 2008; Smith & Trope, 2006), *assignment to high or low power roles* that are not enacted later on (e.g., Galinsky et al., 2003; Guinote, 2007b; Sligte, de Dreu, & Nijstad, 2011), or the *adoption of* a *power-related posture* (e.g., Carney et al., 2010; Cesario & McDonald, 2013; Huang, Galinsky, Gruenfeld, & Guillory, 2011). Among these manipulations, experiential priming is most popular.[2]

Of course, this experimental approach has undeniable strengths such as high internal validity and the causal interpretability of effects (Shadish, Cook, & Campbell, 2002). Moreover, from a practical point of view, the most commonly used priming manipulations are cost- and time-saving to administer, as (1) participants can be tested in groups, (2) experiments can be conducted online and via large participant pools such as MTurk instead of having to laboriously find participants and convince them to come to the laboratory, and (3) there is neither the need to recruit confederates nor to schedule real interaction partners to the same experimental session as would be the case when using role-play manipulations.

However, the predominant approach described above has some limitations which the current dissertation seeks to address. First, experiential priming has been criticized on multiple grounds. Sturm and Antonakis (2014) raised the concern that overtly asking participants to think about power could induce demand characteristics. Tost (2015) suspected that the experiential priming manipulation might also inadvertently prime a strategic orientation, in that participants are likely to recall situations in which their own and the target's interests were in conflict. Both critiques raised the question whether studies relying on this priming procedure really tell us something about the effects of "pure" (psychological) power. Additionally, it might be problematic to ask students, i.e., people who

[2] In addition, role-play manipulations have been used, involving both imagined (e.g., Overbeck & Park, 2001, 2006; Overbeck & Droutman, 2013) and real interaction partners (e.g., Gonzaga et al., 2008; Schmid Mast et al., 2009). However, these "social" power manipulations constitute the minority in published research.

probably have limited experience with structural power, to recall situations in which they actually had power (Lammers, Dubois, Rucker, & Galinsky, 2017).

Second, the relationships between previously identified facets of power have been neither well differentiated nor well integrated. For instance, a few recent theoretical and empirical works suggest that it is important to draw a distinction between *structural power*, i.e., the objectively demonstrable control of valued resources tied to a stable hierarchical position, and *psychological power*, i.e., a psychological state characterized by the activation of a power-specific set of cognitive and behavioral tendencies: Tost (2015) elaborated on the individual and situational conditions under which structural power is likely to result in psychological power. In addition she explained how the effects of structural power are conveyed via a sense of responsibility to produce different outcomes from the ones that have emerged with respect to the dynamics of psychological power. Empirical data supported Tost's distinction between structural and psychological power in that positional power (= structural power) imperfectly predicted feelings of power (= psychological power) and both kinds of power had independent effects on various state measures (Smith & Hofmann, 2016). However, this distinction is rarely made clear and most articles discuss effects of *power* instead of being precise in the sense that, given the manipulations used, they can only draw conclusions regarding *psychological power*.

Third, the dynamic nature of power has mostly been neglected in previous power research (for rare exceptions see Cook, Arrow, & Malle, 2011; Smith & Hofmann, 2016), although individuals do change between high power and low power positions depending on their respective interaction partners (Leikas, Lönnqvist, Verkasalo, & Nissinen, 2013; Schmid Mast, 2010). For instance, in organizational reality, it is possible to compare managers with assistants, as one would do in a classical power experiment. However, this comparison might be limited in informational value because managers as well as assistants will interact with other people occupying positions on different levels of the organizational hierarchy in one day: the "high power" manager is in a subordinate role when discussing an investment with a superior. Hence, the between-person approach that is commonly used limits the adequacy of inferences about higher order constructs (construct validity).

Fourth, studying power exclusively in the laboratory limits external validity and means overlooking important concomitants present in the real world. For instance, real world power is often inextricably tied to responsibility for people with thoughts and feelings with whom the powerholder is often at least acquainted. Moreover, being powerful or being the subject of influence in everyday life has real, tangible consequences - which is not taken into consideration in the prototypical laboratory study.

Current Dissertation

This dissertation investigates the effects of social power on self-control. As I have detailed above, the two major power theories, the approach/inhibition theory (Keltner et al., 2003) and the social distance theory (Magee & Smith, 2013), make conflicting predictions regarding the effect of power on self-control. Empirical findings are equally contradictory: One the one hand, participants in the experimental high power conditions persist longer, make more attempts to solve (unsolvable) tasks (Guinote, 2007a), and are better able to focus on the task at hand (Guinote, 2007b; Smith et al., 2008). On the other hand, they are worse in suppressing thoughts (Guinote, 2007c) and withstanding impulses to act (Scholl & Sassenberg, 2015). Tables 1.1 and 3.2 in the Appendix present comprehensive overviews on studies about the relationship between social power and self-control, with Table 1.1 focusing on descriptive characteristics of the studies and with Table 3.2 focusing on conflicting results.

Inspired by the inconsistent pattern of results, I test the idea that the theoretically and empirically open question of the relationship between power and self-control can be solved by considering self-control as a two-dimensional construct consisting of both predominantly initiatory (start) and predominantly inhibitory (stop) components that are differentially affected by social power. Hermann (1996), in his commentary on a limited resource account in major patterns of self-control failure, was one of the first to point to the fact that self-control failure is not only a problem of stopping, but also a problem of starting. In fact, trait self-control measures have repeatedly been reported to yield a two-factorial structure consisting of a stop and a start component (de Boer, van Hooft, & Bakker, 2011; de Ridder, de Boer, Lugtig, Bakker, & van Hooft, 2011; Maloney, Grawitch, & Barber, 2012), or a three-factorial structure also including a maintenance component (Hoyle & Davisson, 2016).

This taxonomy based on functional processes has also received support regarding its predictive validity. In the prediction of contextual performance at the workplace, only start self-control was positively related to organizational citizenship behavior, personal initiative, and proactive coping whereas both stop self-control and start self-control were negatively related to counterproductive work behavior (de Boer, van Hooft, & Bakker, 2015). In an attempt to model self-control at a broader societal level, indices of stop and start self-control derived from a variety of mundane behaviors at the level of U.S. states were shown to exhibit unique patterns of association with a wide range of outcomes, including homicide, suicide, home foreclosures, divorce, and infidelity (Findley & Brown, 2017).[3]

[3] In addition, as these examples of concrete self-control-related behavior suggest, start and stop self-control are not only important in the *intra*personal but also in the *inter*personal domain. Self-control is needed to maintain interpersonal relationships, i.e., for focusing attention on one's interaction partner instead of own thoughts or distractions, overriding selfish and self-interested impulses, or to control own emotions in order not to hurt or frighten the other (Olson, 2005; Vohs et al., 2009). However, the focus of this dissertation is not on interpersonal self-control and it is hence only investigated in the field study (see Chapter 5).

I hypothesize that social power benefits task performance if the task requires initiatory (start) self-control but impairs task performance if the task requires inhibitory (stop) self-control. I build my argumentation solely on the motivational account advanced within the approach/inhibition theory because the social distance theory is far less broad in focus and only allows for predictions in cases when self-control is conceptualized as the struggle between smaller, sooner rewards and larger, temporally distant rewards. The effects of power on affect and attentional focus are most important for my reasoning, with BAS activation being related to positive affect and attention to rewards and BIS activation being related to negative affect and attention to threats.

I begin by pointing out a fundamental assumption: I am interested in self-control requiring instances in which an individual *consciously* makes the choice to exert self-control (in contrast to effortless self-control, e.g., Gillebaart & de Ridder, 2015). I posit that this kind of self-control is taxing, be it initiatory or inhibitory, individual or interpersonal. This claim is corroborated by research using the dual-task paradigm common in the ego depletion literature, where the self-control performance in a second task is usually worse after the completion of an initial self-control-requiring task (Hagger et al., 2010).

My central assumption is that because the exertion of self-control is taxing (be it within the framework of the process model of ego-depletion, Inzlicht & Schmeichel, 2012, or the strength model of self-control, Baumeister et al., 2007), people in different hierarchical positions concentrate their self-control efforts on the kind of self-control that best fits their motivational focus and their emotional state. As low power people are attentive to threat, punishment, and others' interests, they should be focused on not doing anything wrong, which can be achieved by not violating norms, not following impulses, down-regulating feelings, and thinking things through – which are all examples of what I term inhibitory self-control. Indeed, results by Dholakia, Gopinath, Bagozzi, and Nataraajan (2006) show that the higher the prevention focus (operationalized by BIS), the less respondents reported acting on their temptation. Additionally, the experience of lacking power is related to negative mood, which has been shown to correlate positively with stop self-control (de Boer et al., 2011).

In contrast to low power people, high power people are attentive to rewards. One way to be rewarded is to strive for the attainment of self-relevant, self-set goals. In order to successfully achieve one's goals, it is necessary to turn plans into action, concentrate on the goal and nothing else, as well as persist and make multiple attempts even in the face of obstacles – which are core characteristics of what I term start self-control. In addition, the positive effect of high power on start self-control is further assisted by the positive affect that accompanies BAS activation. For instance, positive affect was positively correlated with start self-control in a self-report study (de Boer et al., 2011). In several studies, in which self-control was operationalized as intertemporal choice, a paradigm we classify as requiring start

self-control, participants in positive affect were more likely to choose a larger mail-in rebate over a smaller instant rebate and do not discount the value of delayed outcomes (in comparison to people in neutral affect; Pyone & Isen, 2011).

Outline

The research question on the nature of the relationship between power and self-control is directly addressed in chapters 2, 3 and 5. Chapter 2 presents a direct replication attempt of the finding that power reduces temporal discounting (i.e., improves self-control; Joshi & Fast, 2013). This finding was chosen for replication because two highly standardized paradigms (episodic priming and temporal discounting) were used in this article. Being able to replicate this finding was meant to constitute a "calibration" for my subsequent research. I was not able to replicate this effect and suspected that the reason might be power priming's inability to reliably affect participants' sense of power that was thought to be responsible for this effect.

In the light of power priming's problems, Chapter 3 reports the development of a new and more effective role-play power manipulation that is based on another highly standardized paradigm known from stress research, the Trier Social Stress Test (TSST, Birkett, 2011). Additionally, in order to examine the idea of self-control as a multidimensional construct, four different but also established self-control measures capturing the rather initiatory or rather inhibitory components were administered. It was not possible to use the temporal discounting paradigm again in this context, as it cannot be classified unequivocally as initiatory or inhibitory. Although the power manipulation strongly affected participants' sense of power, it did not affect self-control performance. This is surprising given that this study can be considered a conceptual replication of previous power research.

Given these two unsuccessful (replication) attempts to find effects of power on self-control in the laboratory, a likely conclusion might be that the published literature has overestimated these effects. This is even more plausible considering that the personal relevance of self-control performance (and also power) in published literature was rather low (cf. de Ridder, Kroese, & Gillebaart, 2018).

To ensure that power and self-control are meaningful for participants, the relationship between the two constructs should be investigated in real people in everyday life. Studying power in real life has two peculiarities: First, it likely changes *within* person, depending on the respective (interaction) situation. Second, conceptual clarity about different forms of power (structural power as objectively demonstrable control over valued resources vs. psychological power as conscious subjective judgment about one's own ability to influence others in a given social situation) is indispensable. As literature on effects of within-person power changes is scarce (for rare exceptions see Goodwin, Gubin, Fiske, & Yzerbyt, 2000; Li, Wang, Yin, Mao, Zhu, & Huang, 2016; Sivanathan, Pillutla, & Murnighan, 2008; Weick,

McCall, & Blascovich, 2017, Study 2), the laboratory study presented in Chapter 4 was conducted to gain insights into within-person changes of structural and psychological power and possible downstream consequences. Given the prior difficulties to find effects on self-control, risk-taking and interpersonal accuracy were used as dependent variables because (1) previous research has reported less ambiguous effects in their case and (2) both can be seen as relevant components of responsible behavior. Although the again newly developed power manipulation did not produce the hypothesized effects on the dependent variables, it did affect psychological power.

Chapter 5 presents a theoretical framework linking objective power and psychological power to self-control in everyday life. We conceptualize power as a multidimensional construct consisting of objective power (i.e., structural power, situational power) and psychological power (i.e., sense of power, personal power, sense of responsibility). Results of an experiment and an experience sampling study, both conducted with non-student samples, supported the key predictions of our theoretical framework: Situational power was positively related to sense of power, which in turn was positively related to personal power and sense of responsibility. We adopted the idea of self-control as a multifaceted construct and investigated effects of this objective-subjective power link on start, stop, and interpersonal self-control. In contrast to the laboratory study presented in Chapter 3, real world power affected real world self-control. However, the effects were inconsistent across the two studies.

Finally, Chapter 6 includes the General Discussion of the empirical evidence presented in Chapters 2, 3, 4 and 5. The findings are summarized, and strengths and limitations are discussed. The chapter closes with implications of the current findings for research on social power and research on self-control, and implications for future research are discussed.

Please note that all empirical chapters are written in a way that allows for them to be read independently of each other and the general introduction. As the predictions derived in Chapter 3 and 5 build on similar theoretical assumptions, these two chapters show some theoretical overlap. Additionally, I would like to stress that the empirical chapters refer to “we” instead of “I” regarding the authors, as the research reported in these chapters was conducted in collaboration. Finally, this dissertation adheres to open science standards. Preregistrations, materials, and data for all reported studies can be accessed via the Open Science Framework: https://osf.io/rykjp/

CHAPTER 2: Does Power Increase Self-Control? Episodic Priming May Not Provide the Answer.

Abstract

Powerful people (e.g., political and business leaders) should be able to control their impulses and act in line with long-term rather than short-term interests. However, theories of power suggest different answers to the question whether the basic experience of feeling powerful decreases (e.g., Keltner, Gruenfeld, & Anderson, 2003) or increases self-control performance (e.g., Magee & Smith, 2013). We conducted a pre-registered direct replication of the only experiment testing the effects of power on self-control (Joshi & Fast, 2013, Study 3). In contrast to the original results, social power, operationalized by episodic priming, did not affect temporal discounting. A possible explanation is the fact that the power priming failed to elevate participants' sense of power. Thus, the null findings challenge the power priming paradigm rather than the two theories from which opposite predictions were derived. In order to understand how power affects self-control, future research may need to rely on other manipulations.

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Introduction and Theoretical Background

How does feeling powerful prepare individuals for exercising self-control, i.e., to pursue long-term goals? Laypeople seem to agree that powerful people such as organizational or political leaders should be particularly persistent, disciplined, and responsible (Lord, Foti, & Vader, 1984). Two influential theories in power research – the approach/inhibition theory of power (Keltner et al., 2003) and the social distance theory of power (Magee & Smith, 2013) – make opposite predictions with regard to the effects of power on self-control.

Within the framework of the approach/inhibition theory of power, Keltner and colleagues suggest that (1) high power activates the behavioral approach system which is sensitive to rewards and opportunities, and (2) low power activates the behavioral inhibition system which is sensitive to punishment, threat, and uncertainty. Briefly summarized, Keltner and colleagues propose that high power triggers approach-related positive affect, attention to rewards, automatic cognition, and disinhibited behavior, whereas reduced power activates inhibition-related negative affect, systematic cognition, and situationally constrained behavior. Accordingly, due to their heightened attention to rewards and their drive to experience these rewards immediately, powerful people should show relatively poor self-control.

In contrast, the social distance theory of power (Magee & Smith, 2013) assumes that high-power individuals exhibit better self-control than low-power individuals. Magee and Smith propose that asymmetric dependence between two individuals gives rise to asymmetric experiences of social distance, with the high-power individual feeling more subjective distance than the low-power individual. Based on assumptions of construal level theory (Trope & Liberman, 2010), the authors assume that because high-power individuals perceive larger social distance, they engage in more abstract mental representation (i.e., higher level construal) than low-power individuals. High-level construals have been shown to have a positive effect on self-control (e.g., Fujita, Trope, Liberman, & Levin-Sagi, 2006; Schmeichel, Vohs, & Duke, 2011). Accordingly, due to their use of high-level construal of goals and situations, powerful people should show good self-control.

In line with the assumptions of the social distance theory of power, Joshi and Fast (2013) showed in three studies that experimentally induced social power benefits the pursuit of long-term goals by reducing the preference for smaller immediate gains over larger future gains (i.e., temporal discounting). When two theories make different predictions and only one is supported, the question arises as to what extent the other theory should be modified or discarded. Given the practical importance of self-control among powerful individuals, research must identify the conditions under which one or the other theory is correct.

However, considering that the only available evidence on the research question comes from a single lab, it seems reasonable to first ask if the effect found by Joshi and Fast (2013)

is robust before future research can systematically explore moderators of the effect. This first step is an important one: As all findings result from a combination of signal (an underlying effect) and noise (systematic error in form of moderators as well as unsystematic error in form of measurement error), direct replication is the only way to separate the noise from the signal and average across different types of error (Simons, 2014).

The current work was an attempt to replicate Study 3 from Joshi and Fast (2013). This study was chosen for two reasons. First, it is the only one that had a 3-cell design (high power, low power and control condition) and would therefore show whether the effect is attributable to high or low power (Singh, 1998). Second, it was assumed that preferences for gains in air quality (nonmonetary temporal discounting) would be more comparable across industrialized nations (USA vs. Germany/Switzerland) than preferences for monetary rewards where differences in currency, purchasing power, and inflation might play a role.

In the original web-based study, 78 students experienced a power or control priming, then completed a measure of connection with their future self and finally the nonmonetary temporal discounting task. We conducted a pre-registered direct replication study (osf.io/um3rq) based on the Replication Recipe (Brandt et al., 2014) using a substantially larger sample. A successful replication would find a significant effect of the power manipulation, in that participants in the high power condition would have lower discount rates than participants in the neutral and low power conditions.

Method

All study materials and procedures can be accessed via osf.io/dqr4m. The present research was done in accordance with the checklist issued by the responsible ethics committee of the Faculty of Philosophy, University of Zurich, meaning that no formal approval was needed. This research respects the Ethical Principles of Psychologists and Code of Conduct by the American Psychological Association (APA) as well as the Ethics Guidelines for Psychologists by the Swiss Psychological Society.

Participants

Sample size was determined based on considerations of statistical power. Simonsohn (2015) noted that the conventional approach of using the effect size estimate of the original study may be problematic. First, publication bias may inflate published effect sizes. Second, a replication may be uninformative when the confidence interval of the replication effect size does not only include zero, but also a detectable effect, that is, an effect size that the original study could have detected with 33% power. According to the recommendations by Simonsohn (2015) at least 2.5 times as many observations as the original study should be collected to have about 80% power to reject the null hypothesis of a detectable effect (i.e., in this case $N_{Original}$ = 78, minimum $N_{Replication}$ = 195). In the present case, a power analysis

assuming Joshi and Fast's (2013) sample size, equal per cell sample sizes[4] and statistical power of 33% indicated that the minimum detectable effect was equal to Cohen's d = 0.35. The desired sample size was set to $N_{Replication}$ = 258 because this affords 80% power to reject the null hypothesis that the effect is zero if the effect is detectable (i.e., d = 0.35) and (at least) 80% power to reject the null hypothesis of a detectable effect if the effect is in fact zero.

In total, 636 participants gave their consent to participate in the study (210 participants in high power condition, 218 participants in control condition, 208 participants in low power condition). On the third page where participants were meant to experience the power priming, 129 participants in the high power condition, 69 participants in the control condition, and 103 participants in the low power condition dropped out. Comparing all three groups, the dropout rates differed significantly, $X^2(2) = 38.64$, $p < .01$. Taking only the two power conditions into consideration, the dropout rates still differed significantly, $X^2(1) = 5.53$, $p = .02$, with 61% vs. 50% dropping out of the study after reading the instructions for the high vs. low power priming. This could be a cause for concern if dropout was systematically related to individual differences. Unfortunately, participants were asked for their demographics (gender and age) only at the very end of the experiment (in line with Joshi & Fast). Accordingly, we were not able to test if gender and condition interact in predicting dropout rates. However, if this was the case we would observe different proportions of men in women in the three conditions, which we do not, $X^2(2) = 0.82$, $p = .66$ (high power: 36% men, control: 40% men, low power: 43% men). Furthermore, if age was confounded with condition, we would observe differences in the mean age across the three conditions, which we do not either, $F(2,278) = 0.04$, $p = 0.96$ (high power: 27.24 years, control: 27.29 years, low power: 27.56 years). Likewise, proportions of students vs. professionals did not differ across conditions, $X^2(2) = 0.09$, $p = .96$ (high power: 45% students, control: 45% students, low power: 47% students). In light of these results, we believe that systematic dropout does not affect the validity of our manipulation.

Finally, several participants were excluded based on the following a priori exclusion rules: no answers to either the questions referring to the connection to the future self or the temporal discounting questions (31 participants), inconsistent discounting pattern (16 participants), no discounting at all (6 participants), no meaningful description of the situation in which they had / did not have power / were shopping (19 participants). In summary, 22 participants in the high power condition, 24 participants in the control condition and 26 participants in the low power condition were excluded. The exclusion rates did not differ significantly across condition, $X^2(2) = 4.768$, $p = .092$. The final sample consisted of 263

[4] For simplicity we report calculations based on the assumption of equal per cell sample sizes because assuming unequal cell sizes does not alter our conclusions.

participants (M_{age} = 27.21 years, *SD* = 7.21 years), 98 men, 147 women and 18 participants of unknown gender.

Materials and Procedure

A convenience sample was invited via snowball sampling to take part in an online study on decision behavior. Participants gave their written consent and were then randomly assigned to one of the following three conditions: participants recalled and wrote about a situation when they had power (high power, 59 participants), or when they lacked power (low power, 79 participants) or when they last went to the grocery store (control condition, 125 participants). Following the power manipulation, participants completed a measure of connection with the future self. Participants selected one of seven, increasingly overlapping pairs of circles to indicate how "connected" and how "similar" they felt to their selves in 10 years. These two items were averaged to form a measure of participant's connection with his/her future self with higher values indicating a stronger connection.

The next part of the study consisted in the nonmonetary temporal discounting task. Within this paradigm, a participant makes a number of choices between a larger and several smaller rewards, where the smaller reward is available sooner than the larger one (Green & Myerson, 2004; Smith & Hantula, 2008). Participants were provided with eight binary choices between "improved air quality immediately for 21 days" and "improved air quality one year from now for [number of] days." The number of days in the future was 21, 23, 25, 27, 29, 31, 33, or 35. A single indifference point for each participant was obtained. This is the point at which participants equally value present and future gains. It was calculated by averaging the number of days between which participants switched from preferring the present option to preferring the future option. A high indifference point represents a tendency to prefer a smaller and more immediate reward or a failure to consider long-term potential consequences.

Next, participants completed the Personal Sense of Power Scale (Anderson, John, & Keltner, 2012). Participants stated their agreement to 8 items such as "I think I have a great deal of power" on a scale from 1 (I disagree) to 5 (I agree). Cronbach's alpha was α = .72. All the original materials for the study were available from the authors. All instructions were direct translations.

After the direct replication, we collected additional measures that are not relevant for present purposes. For more information on the procedure and results related to these measures please consult the separate report on the Open Science Framework (osf.io/j67ep). Finally, participants were asked for their demographics.

A few details differed between our study and that of Joshi and Fast (2013) beyond the obvious differences in language and national context. First, we did not counterbalance the order of the connection with the future self scale and the temporal discounting task because

the former was described as a mediating variable by Joshi and Fast. Second, our sample did not only include students (110 participants) but also professionals (135 participants, no such information available for 18 participants). Although professionals were older ($M_{student}$ = 24.55, *SD* = 2.92; $M_{professional}$ = 29.38, *SD* = 8.77), *t*(169.02) = 6.01, *p* < .01, and reported a slightly higher socioeconomic status ($M_{student}$ = 3.12, *SD* = 0.84; $M_{professional}$ = 3.31, *SD* = 0.94), *t*(241.06) = 1.77, *p* = .08, these two groups were collapsed as the effects in the two groups were similar. Third, in the original experiment participants took part in exchange for course credit. In our replication, participants were invited to take part in a prize draw, in which they could win one of three vouchers worth 30 CHF / 30 EUR (corresponding to approximately 33 USD) each.

Results

Manipulation Check

Two coders, who were blind to both conditions and hypotheses, categorized what type of relationship (e.g., manager–subordinate, teacher–student) was described in participants' responses to the writing prompts. They were instructed to reach agreement for this categorization. Table 2.1 shows that 98.3% of participants in the high power and 92.4% of participants in the low power condition wrote about experiencing power (or the lack thereof) in various relationships. Their responses were rated by the same two coders for how much power the participant reported having using a scale ranging from 1 (none at all) to 5 (very much). The interrater reliability (agreement definition) was good, *ICC* = .84. We used the mean rating of two raters as dependent variable for the manipulation check. Participants described themselves as having more power in the high-power essays (*M* = 4.02, *SD* = 0.58) than in the low-power essays (*M* = 1.39, *SD* = 0.66), *t*(127) = 23.66, *p* < .01.

Table 2.1
Percent of power relationship types described by participants.

Type of relationship	High power condition	Control condition	Low power condition
Manager – subordinate	13.6	0.0	22.8
Parent – child	11.9	0.0	8.9
Peers	8.5	0.0	1.3
Interview/admission	15.3	0.0	16.5
Teacher – student	10.2	0.0	11.4
Friends/relatives	8.5	0.0	3.8
Romantic/dating/sexual partners	6.8	0.0	1.3
Miscellaneous	23.7	0.0	26.9
No power relationship/Unclear	1.7	100.0	7.6

Note. Types of relationships that totaled less than 1% were aggregated to form the category "Miscellaneous".

Preliminary Analyses

A one-factorial ANOVA on the personal sense of power revealed no difference across

conditions, $F(2,259) = 0.28$, $p = .75$, $\eta_p^2 = 0.002$, 95% CI [0, 0.02]. Furthermore, there were no significant differences between conditions in temporal discounting, $F(2,260) = 0.27$, $p = .76$, $\eta_p^2 = 0.002$, 95% CI [0, 0.02], or the felt connection with the future self, $F(2,260) = 0.20$, $p = .82$, $\eta_p^2 = 0.002$, 95% CI [0, 0.02] (for descriptives see Table 2.2).

Table 2.2
Means and standard deviations.

Measure	High power condition (n = 59)	Control Condition (n = 125)	Low power condition (n = 79)
Personal sense of power	3.97 (0.48)	3.91 (0.59)	3.93 (0.47)
Connection with future self	4.30 (1.45)	4.31 (1.44)	4.42 (1.39)
Discount factor	0.39 (0.27)	0.42 (0.26)	0.43 (0.25)

Table 2.3 shows the intercorrelations of all measured variables. Only one correlation was significant: Personal sense of power was positively related to a stronger connection with the future self. Although only correlational evidence, this supports Joshi and Fast's reasoning that high power should increase the connection with the future self.

Table 2.3
Intercorrelations of measured variables.

	Personal sense of power	Connection with future self
Connection with future self	0.23	
Discount factor	0.07	0.08

Note. Correlations that equal or exceed .13 (.16) are significant at the .05 (.01) level.

Main Hypothesis Test

Joshi and Fast (2013) reported a significant contrast between participants in the high-power condition and those in the low-power and baseline conditions combined. The same analysis performed on the present data indicated a non-significant difference in the same direction as the effect reported by Joshi and Fast (2013), $t(261) = 0.71$, $p = .48$, $d = 0.09$, 95% CI [–0.15, 0.33]. Thus, the confidence interval indicates that the effect is consistent with the null hypothesis of no effect but inconsistent with the null hypothesis of a detectable effect (which was determined to be $d = .35$ see above). As for the presumed mediator of the effect observed by Joshi and Fast (2013), high-power participants did not score higher on the measure of connection with the future self than did participants in the other two conditions, $t(261) = 0.33$, $p = .74$, $d = 0.04$, 95% CI [–0.20, 0.28].

Discussion

Replication constitutes an important contribution to cumulative science because it allows for testing the robustness of results and hence provides researchers with greater confidence about the existence and direction of effects (Brandt et al., 2014, Klein et al., 2014). Given the importance of understanding the relationship between social power and self-control, we sought to replicate Joshi and Fast's (2013, Study 3) finding that power increases self-control (d = .53). Using a much larger sample size but nearly identical procedures as Joshi and Fast we obtained a much smaller effect (d = 0.09, 95% CI [–0.15, 0.33]). This non-significant effect is informative because it is significantly smaller than an effect that would give the original study a statistical power of 33% (i.e., the minimum detectable effect; Simonsohn, 2015) and that the current research had 80% statistical power to detect (i.e., d = .35). One straightforward implication of this result is that future replication studies relying on the procedures used by Joshi and Fast (2013) should be prepared to collect even larger samples to achieve adequate statistical power.

Ultimately, though, researchers are likely to be more interested in the validity of the theoretical claims about the effects of social power than in the reproducibility of one particular study using a specific methodology. On the face of it, the null finding regarding the relation of power and discounting would seem to disconfirm the predictions derived from both the social distance theory of power (predicting a negative effect) and the approach/inhibition theory of power (predicting a positive effect). However, every empirical study involves auxiliary assumptions regarding the operations and measures used to test a theory, and those may be wrong as well. Most prominently, the validity of the manipulation and measures might be questioned.

Temporal discounting as operationalization of self-control is a well validated and common paradigm in psychology and economics. It has successfully been used in pathological (e.g., substance addicts, MacKillop, Amlung, Few, Ray, Sweet, & Munafò, 2011; pathological gamblers, Dixon, Marley, & Jacobs, 2003) as well as in normal populations, in student as well as non-student populations (e.g., Buono, Whiting, & Sprong, 2015), in the USA as well as in Germany and Switzerland (e.g., Gianotti, Figner, Ebstein, & Knoch, 2012; Peters, & Büchel, 2009).

Admittedly, the air quality delay discounting task that was employed in the original study differed in two aspects from more widely used versions of this task: the number of delays and the discounted reward. Although it is more common to administer temporal discounting tasks with several delays, the one-shot discounting task chosen by Joshi and Fast should yield comparable results to a more comprehensive version of this task. Reimers, Maylor, Stewart, and Chater (2009) compared a one-shot discounting task with a discounting task with several delays and obtained comparable results. In a similar vein, Yi, Pitcock,

Landes, and Bickel (2010) found that valid and sensitive discounting indices can be obtained with fewer indifference points than the standard number of 5 or 7. Regarding the nature of the discounted reward, an improvement in air quality for 21 days may seem relatively intangible (and maybe irrelevant) in comparison to receiving (hypothetical) monetary rewards for oneself. However, previous studies showed that temporal discounting occurs when environmental rewards are in question (e.g., air and water quality, Guyse, Keller, & Eppel, 2002; nuclear and hazardous wastes, Moser, Stauffacher, Smieszek, Seidl, Krütli, & Scholz, 2013; improvements in green space and storm-water control as well as reducing greenhouse gasses, Richards & Green, 2015). Furthermore, in we did not find an effect of social power on monetary temporal discounting either[5] (see supplementary material available at osf.io/j67ep).

Regarding the manipulation of power, it must be noted that the episodic power priming is also a widespread paradigm (Galinsky, Rucker, & Magee, 2015) that has already been successfully used in German and Swiss samples (e.g., Schmid & Schmid Mast, 2013; Scholl & Sassenberg, 2014a). In fact, noting that sometimes one and the same article reports similar findings across studies with priming or role-based manipulations, power researchers have argued for the superiority of priming manipulations because it "remove[s] issues of conscious awareness or intent" (Smith & Trope, 2006, p. 580) or because "it can be difficult to manipulate power in an ethical, believable, and effective way in the laboratory" (Smith & Galinsky, 2010, p. 928).

However, we think it is possible that the simplicity of this paradigm is offset by its inability to reliably produce a sense of power that would affect participant's decision-making – at least when administered online. Although our manipulation check indicated that participants followed instructions and provided examples of situations that differed in experienced power, a closer look at the properties of the texts produced by participants reveals that the intensity of the manipulation may have been rather low. Participants in this online study wrote on average 264 characters (*SD* = 246, range from 13 to 2352 characters) and took about Mdn = 197 seconds to do so. Assuming that the average reading speed (German, aloud) is 11.5 characters per second (*SD* = 5.5, Trauzettel-Klosinski & Dietz, 2012) and the average typing speed is 2.82 characters per second (Soukoreff & Mackenzie, 1995), participants would have needed about 118 seconds on average for reading and writing and accordingly would have had 79 seconds left to find a suitable situation and put themselves in this situation. This might not have been enough time to really experience the imagined situation. In fact, the differences in sense of power that we observed across conditions were non-significant and negligible in terms of effect size. Although this may in part be due to the fact that we did not modify the original items so that they would explicitly refer to the current

[5] Note that comparisons with the monetary delay discounting task have to be interpreted with caution as due to dropouts the sample is much smaller than in the main analyses.

situation (as opposed to the dispositional sense of power), it is noteworthy that a standard power manipulation did not leave its mark on a reliable measure of felt power.

In order to enhance the effectiveness of the episodic priming manipulation, a reviewer suggested freezing the survey on the manipulation page and asking participants to visualize the respective situation (e.g., by adding the prompt to picture the faces of the other people involved, to imagine talking with the person and to try to feel the other people there with them). We agree that these additional instructions may serve to intensify the priming manipulation. Another explanation for our inability to replicate the effect of power on self-control could be a weak explicit concept association between social power and self-control. Given that priming occurs as a result of spreading activation of related concepts in memory, larger effects should be found for strongly associated concept pairs (Salomon, 2016). We have reason to believe that the concepts of power and self-control might be only weakly associated (data from an unrelated pilot study).

In summary, as the power manipulation used here did not affect felt power, it seems fair to begin by questioning the superiority of the episodic priming paradigm rather than by concluding that there is no effect of social power on self-control. Perhaps the claim that “[a]ll the ways of manipulating power seem to have similar effects“ (Smith & Galinsky, 2010, p. 928) should be evaluated more systematically, either through meta-analysis or via pre-registered comparisons. The alternative hypothesis would be that some of the effects of power require more intense feelings of power and/or conscious awareness of being in a powerful or powerless situation. Future research using different manipulations and operationalizations of both constructs is needed to clarify this effect and help adjudicate between the opposite predictions regarding the direction of the effect. The present research suggests that episodic priming may not be an ideal vehicle for this effort.

CHAPTER 3: Social Power and Dimensions of Self-control: Does Power Benefit Initiatory Self-control but Impair Inhibitory Self-control?

Abstract

People in power positions should be able to control their impulses and act in line with long-term goals. However, two influential theories disagree as to whether power is conducive (Magee & Smith, 2013) or detrimental (Keltner, Gruenfeld, & Anderson, 2003) to exercising self-control. We propose to resolve this contradiction by distinguishing between initiatory ('start') and inhibitory ('stop') self-control components that may be differentially affected by social power. Ninety-five female participants were randomly assigned to either a powerful role (interviewer) or a powerless role (applicant) and interacted in a simulated job interview (i.e., a modified Trier Social Stress Test). They then completed two inhibitory (d2 Test of Attention and emotion regulation) and two initiatory (handgrip and creative problem solving) self-control tasks. We tested the hypotheses that social power benefits task performance if the task requires start self-control but impairs task performance if the task requires stop self-control. Although the power manipulation strongly affected participants' sense of power, it did not significantly affect self-control performance. Considering that this preregistered study had 80% power to detect an effect of d = .64, we conclude that the population effect size is smaller than that.

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Introduction and Theoretical Background

In general, the stereotypical view of powerful people's self-control performance is inconsistent. On the one hand, we tend to see powerful people as following their impulses – often mentioned examples are sex scandals involving top-ranking politicians (e.g., Bill Clinton, Dominique Strauss-Kahn). On the other hand, it belongs to the image of powerful people that they are always thinking at least 10 steps ahead and aligning their actions with strategic long-term goals (such as Francis Underwood (portrayed by Kevin Spacey) in the popular TV series *House of Cards*). The question arises as to whether experiencing social power (i.e., control over the material and immaterial resources of others; see Galinsky, Gruenfeld, & Magee, 2003; Keltner et al., 2003) benefits or impairs willingness (not ability) to show self-control.

Psychological research to date is not able to answer this question. The social distance theory of power (Magee & Smith, 2013) postulates that power should improve self-control performance. In contrast, the approach/inhibition theory of power (Keltner et al., 2003) suggests that power should worsen self-control performance. Empirical findings are similarly contradictory. For one, participants in high-power conditions are better able to focus on the task at hand (Guinote, 2007b; Smith, Jostmann, Galinsky, & van Dijk, 2008), and they persist longer and make more attempts to solve (unsolvable) tasks (Guinote, 2007a) than participants in low-power conditions. But for another, they are worse at suppressing thoughts (Guinote, 2007c), withstanding impulses to act (Scholl & Sassenberg, 2015), and they take more risks (Anderson & Galinsky, 2006).

If self-control is a unitary construct, these findings cannot all be true. We propose to resolve this contradiction by considering self-control to be a two-dimensional construct consisting of both initiatory and inhibitory components. We call the predominantly initiatory component *start self-control* and propose that it is needed for initiating and maintaining goal-directed behavior. *Stop self-control*, the predominantly inhibitory component, is needed for suppressing behavior or refraining from acting impulsively.

In the following, we will first develop our two-dimensional view of the self-control construct and then review previous research on power and self-control with this distinction in mind. We then test the hypotheses that social power benefits task performance if the task requires start self-control but impairs task performance if the task requires stop self-control.

Operationalization and Dimensionality of Self-Control

Self-control (also called self-regulation, willpower, effortful control, among other terms; Duckworth & Kern, 2011) has been defined as "the capacity for altering one's own responses, especially to bring them into line with standards such as ideals, values, morals, and social expectations, and to support the pursuit of long-term goals" (Baumeister, Vohs, & Tice, 2007, p. 351). Impulsivity can be seen as the opposite of self-control. It describes "the

tendency to act on a whim and, in so doing, disregard a more rational long-term strategy for success" (Madden & Johnson, 2010, p.11) and "has been defined variously as an inability to wait, a tendency to act without forethought, insensitivity to consequences, an inability to inhibit inappropriate behaviors" (Reynolds, Ortengren, Richards, & de Wit, 2006, p. 306).

To better understand the inconsistent findings regarding the effects of power on self-control, we turned to the operationalization of self-control and impulsivity. For measurement of both impulsivity and self-control there are psychometric and experimental approaches. Examples of the psychometric approach are the Eysenck Impulsiveness Scale (Eysenck, Easton, & Pearson, 1984), Self-Control Scale (Tangney, Baumeister, & Boone, 2004), and Barratt Impulsiveness Scale Version 11 (Barratt, 1985). Typical tasks administered in the laboratory are tasks targeting executive functions, such as the Go/No-Go task, Stroop task, or delay of gratification/delay discounting tasks (Smith & Hantula, 2008).

The idea that self-control measures may reflect a smaller number of higher-order constructs is not new. Looking at both psychometric and experimental approaches, we find several classifications for both. Table 3.1 in the Tables section presents an overview of the different conceptualizations. First, self-report measures have repeatedly been reported to yield a two-factorial structure (De Boer, Van Hooft, & Bakker, 2011; de Ridder, de Boer, Lugtig, Bakker, & van Hooft, 2011); those authors first proposed to split the self-control construct into an *inhibitory* and an *initiatory* component. A recent examination of the factor structure of the Brief Self-Control Scale (Maloney, Grawitch, & Barber, 2012) revealed a structure consisting of two significantly negatively correlated factors, referred to as *restraint*, "the tendency to resist temptation" (p. 113), and *impulsivity,* "acting on spontaneous thoughts and feelings" (p. 113).

Second, behavioral tasks can also be classified according to different taxonomies. Hagger, Wood, Stiff, and Chatzisarantis (2010) proposed two content-related classifications of tasks (without testing them empirically): (a) according to the demands placed on *cognitive* or *affective processing* systems, and (b) according to task content (*controlling attention*, *emotions*, *thoughts*, and *impulses*; *cognitive processing*; *choice and volition*; and *social processing*). The idea of multidimensionality of behavioral self-control measures also received empirical support. A meta-analytic principal components factor analysis demonstrated that laboratory tasks typically used to measure impulsivity constitute four factors: *inattention*, *inhibition*, *impulsive decision-making*, and *shifting* (Sharma, Markon, & Clark, 2014).

Third, taking both self-report measures and behavioral tasks into consideration simultaneously, multidimensional conceptualizations emerge as well. A meta-analysis by Duckworth and Kern (2011) used a fourfold classification of self-control measures (*executive functioning*, *delay of gratification/temporal discounting*, *self-report* and *informant-report*

questionnaires). Work on primary data usually finds that self-report measures load on a single factor, whereas the behavioral measures constitute more than one factor:

- *Self-report, impulsive decision-making*, *impulsive disinhibition* (Reynolds, Ortengren, Richards, & de Wit, 2006)
- *Self-report, impulsive decision-making*, *impulsive disinhibition*, and *impulsive inattention* (Reynolds, Penfold, & Patak, 2008)
- *Self-report, response inhibition*, *delay of reward* (Lane, Cherek, Rhoades, Pietras, & Tcheremissine, 2003)

The Start/Stop Distinction

The evidence presented above clearly supports the idea of multidimensionality, although the taxonomies differ considerably. Self-report measures seem to be categorized best by a simple functional taxonomy. In contrast, behavioral measures seem to be organized best according to more multifaceted taxonomies based on task content. Here we propose to apply the functional distinction between *inhibitory* and *initiatory self-control* to organize the variety of behavioral measures. We acknowledge that a simple dichotomy may not suffice to explain method variance, but it may constitute an important step forward in developing the theoretical link between power and self-control.

These two dimensions have already received preliminary empirical support. For instance, within the health domain, inhibitory self-control seems to be important for behaviors that require stopping a response, such as limiting intake of foods high in saturated fat, whereas initiatory self-control (updating) is important for carrying out behaviors that require the initiation of a response, such as consuming fruit and vegetables (Allom & Mullan, 2014). In the organizational context, De Boer, van Hooft, and Bakker (2015) drew on these two types of self-control to predict contextual performance at the workplace: Results showed that only initiatory control was positively related to organizational citizenship behavior, personal initiative, and proactive coping. Both inhibitory control and initiatory control were negatively related to counterproductive work behavior.

Given this initial evidence for the predictive validity of the proposed two factor conceptualization, we categorize laboratory tasks used to measure tendency to act without thinking and suppression of impulses as behavioral operationalization of inhibitory (stop) self-control and laboratory tasks measuring persistence or capability to overcome one's weaker self as operationalization of initiatory (start) self-control. As noted above, within the research on social power and self-control there are numerous theoretical and empirical contradictions. We propose to begin organizing the different findings in the literature by explicitly considering previous classifications of self-control in other areas of research.

Effects of Social Power on Dimensions of Self-Control

The two most influential theories of power make different predictions regarding the effects of power on self-control. More specifically, according to the approach/inhibition theory of power (Keltner et al., 2003), high power activates the behavioral approach system, which is sensitive to rewards and opportunities. Hence, high power should trigger approach-related positive affect, attention to rewards, automatic cognition, and disinhibited behavior. Correspondingly, due to the powerful people's heightened attention to rewards and their drive to experience these rewards immediately, they should show relatively poor self-control.

According to the social distance theory of power (Magee & Smith, 2013), high-power individuals feel more subjectively distant from others than low-power individuals. Based on assumptions of construal level theory (Trope & Liberman, 2010), this greater perceived social distance should lead to more abstract mental representation (i.e., higher level construal). High-level construal was shown to benefit self-control (e.g., Fujita, Trope, Liberman, & Levin-Sagi, 2006; Schmeichel, Vohs, & Duke, 2011). Accordingly, due to their use of high-level construal of goals and situations, powerful people should show relatively better self-control.

When two theories make different predictions and there is empirical evidence in support of both, the question arises as to what the conditions are under which one or the other theory is correct. We used the distinction between start self-control and stop self-control to structure published findings on power and self-control (for a detailed overview, see Table 3.2 in the Tables section). It appears that regardless of the power manipulation (episodic priming, conceptual priming, not enacted role assignment, impact of opinion), participants in the high power condition showed better performance in start self-control tasks such as dichotic listening task, problem-solving tasks, Stroop task, and temporal discounting, whereas in most cases they performed worse in stop self-control tasks such as thought suppression, action planning, and deliberation tasks. We decided not to include findings on powerful people's increased risk-taking propensity (e.g., Anderson & Galinsky, 2006; Carney, Cuddy, & Yap, 2010; Jordan, Sivanathan, & Galinsky, 2011), because risk-taking is not a common operationalization of the self-control construct. However, as risk-taking is sometimes used as a measure in impulsivity research and entails acting without prior deliberation, we consider this to be indirect evidence in favor of our hypothesis of reduced stop self-control in powerful individuals.

Method

All study materials and procedures can be accessed via https://osf.io/u9xa2/. This study was approved by the responsible ethics committee of the Faculty of Philosophy at the University of Zurich. Participants gave their written consent to take part in the study.

Power Analysis

We planned to recruit 100 participants, but time and resource constraints only allowed us to collect data from 95 participants. A power analysis suggested that this sample size affords 80% statistical power to detect an effect of at least $d = .64$ with an experimentwise alpha of 5%, assuming one-sided tests. We will therefore declare an effect significant if it is in the expected direction and $p < .025$ (note that this criterion follows from one-sided testing and spreading the experimentwise alpha error across four hypothesis tests that follow from our use of four self-control tasks). Otherwise we will conclude that the effect is less than the minimal detectable effect size.

Participants

We recruited 95 women in the age range 19 to 48 years ($M = 25.36$, $SD = 6.12$; 82 students) from the pool of psychology students and interested community members at the University of Zurich to take part in a study on personality and interpersonal behavior. We decided in favor of an all-female sample to: (1) avoid possible confounds induced by mixed gender interactions in the role play task, and (2) minimize the risk of self-handicapping effects (e.g., in the creative problem solving task), as these effects seem to be less pronounced for women than for men (McCrea, Hirt, & Milner, 2008). Participants were paid either 30 Swiss francs (about 30 U.S. dollars) or partial course credit. One participant misunderstood the creative problem solving task (i.e., rather than crossing the lines, she retraced the lines) and was therefore excluded only for the analyses of the creative problem solving data.

Procedure

Before coming to the laboratory, participants were asked to complete an online questionnaire that contained questions on demographics, personality (BFI-15; Gerlitz & Schupp, 2005), and potential moderator variables (trait self-control: De Boer et al., 2011; implicit theories about willpower: Job, Dweck, & Walton, 2010; explicit power motive on the *Inventar zur Erfassung interpersonaler Motive* (IIM): Thomas, Locke, & Strauss, 2012).

When they arrived at the laboratory, participants learned that they and another participant (who was in fact a confederate) were scheduled for the same experimental session and would work on one task (among others) together. At the very beginning of the first part of the experimental session, a female experimenter first administered the baseline measure of the handgrip task for each participant separately. Then, a modified version of the Trier Social Stress Test (TSST; Birkett, 2011) was used to manipulate the feeling of power. Participants were randomly assigned to a powerful role (as interviewer) or a powerless role (as applicant) and interacted in a simulated job interview (the self-presentation part of the TSST) with the female confederate. At the end of the first part of the experimental session,

participants completed a manipulation check and several filler questions concerning mood and feelings during the interaction. The first part of the session took approximately 25 min.

To guarantee that the experimenter who administered the dependent measures was not aware of the experimental condition, participants were instructed by a second female experimenter to complete four well-established self-control measures: For stop self-control we used the d2 Test of Attention (Brickenkamp, 1994) and an emotion regulation task (avoiding emotional displays and facial expressions while watching a funny, a distressing, and a boring short film in counterbalanced order). For start self-control, we used the handgrip task and an ostensible test of creative problem solving abilities. These four tasks were administered in four different orders, so that every task was once in the first, the second, the third, and the last position. Subsequently, participants completed a second manipulation check and several filler questions concerning mood and their overall impressions of the experiment. Participants received their compensation and were debriefed and thanked. The second part of the session took approximately 50 min.

Power Manipulation

To create a highly involving and naturalistic hierarchically structured situation we used a modified version of the TSST. The TSST generally consists of a waiting period upon arrival, anticipatory speech preparation, speech performance, and verbal arithmetic performance periods, followed by one or more recovery periods. Our implementation of this paradigm differed in the following main aspects from the standard procedure (see Birkett, 2011): First, we were not interested in assessing stress hormone reactivity, so we omitted both the waiting period at the beginning and the verbal arithmetic performance period at the end. Second, the interviewers wore their normal clothes instead of lab coats to keep the situation more naturalistic and provide a better fit with the cover story.

Participants believed that the dyadic task was a mock interview in which one person would play the role of the interviewer, a professional recruiter, and the other person the role of the job applicant. The research assistant would play the role of assistant to the interviewer. Depending on the condition, the real participant would either act as the interviewer (high power role) or the applicant (low power role). Participants were informed that the applicant had 7 min to mentally prepare a 4-min speech in which she presents herself as an ideal candidate for her dream job. Her speech was videotaped. In the preparation period, the interviewer had to determine evaluation criteria for the presentation and could prepare up to three questions for the applicant. Participants believed that the role allocation was randomly determined – in fact, it was randomly determined before the experiment started, and the real participant drew either one of two interviewer lots or one of two applicant lots.

The research assistant brought the applicant to an adjacent room where she had to mentally prepare her self-presentation. Applicants had to use an annoyingly ticking egg timer

to monitor their 7 min preparation time. They were informed that the interviewer would pick them up for the presentation.

In the meantime, the interviewer and the research assistant prepared the setting, arranging chairs and table so that both of them sat on the same side of the table, placing the chair for the applicant to face them, and positioning the video camera. When the interviewer was a real participant (i.e., in the high power condition), she was given written instructions that summarized the role requirements in order to make her feel as comfortable as possible in her role. The instructions summarized the interviewer's goals (find out possible strengths and weaknesses, evaluate the quality of the presentation) and procedural rules (e.g., ask for what job the applicant is applying, do not interrupt the presentation, prompt the participant to continue speaking if she remains silent for more than 10 sec, take notes if needed, prepare up to 3 questions).

Then participants played their respective roles: The interviewer welcomed the applicant, the applicant gave her speech and answered questions, and the interviewer thanked the applicant and brought her back to the preparation room. The interviewer and the research assistant took approximately 3 min to discuss the presentation in a way that allowed the interviewer to feel like the person in charge. After this discussion, the interviewer was asked to evaluate the applicant. The interviewer was informed that her evaluation was important because the applicant's chances of winning a bonus would depend on the evaluation, whereas the interviewer's bonus would be randomly decided.

Measures

Manipulation checks

After the interview role play, the power manipulation, we assessed how powerful and in charge of the situation each participant felt during their interaction. On a scale from 0 (not at all) to 5 (completely) participants indicated how much they agreed with six self-descriptive adjectives: "powerful," "self-confident," "unassertive" (reverse-coded), "subordinate" (reverse-coded), "responsible," and "competent." These items were averaged to build an indicator of felt power. Cronbach's alpha was $\alpha = .81$. At the very end of the experiment, we asked the participants again how powerful they felt (0 = not at all, 5 = completely) in order to have an indicator of the stability of the power manipulation.

Behavioral measures

We used four tasks to represent the two self-control components. We chose well-established measures in the self-control and ego-depletion literature (e.g., Hagger et al., 2010) that are easy to administer. To our knowledge, these tasks have not yet been used in conjunction with any power manipulation.

Handgrip task. This task was used as an operationalization of start self-control. We implemented this task based on the description by Muraven, Tice, and Baumeister (1998).

The apparatus used for this task was a hand exerciser consisting of two handles and a metal spring. Participants were told to squeeze the handles together and maintain that grip for as long as they could with their dominant hand. A small eraser was inserted between the handles so that when the grip relaxed the eraser would fall down, thereby providing a clear audio-visual and objective signal to stop timing. The experimenter timed how long the participant squeezed the handles (i.e., endurance in seconds). Participants completed a baseline measure at the beginning of the experiment and a second measure after the power manipulation.

We conceptualize persistence as an integral component of start self-control. The handgrip task requires physical stamina and accordingly becomes taxing with time. Therefore, a person must exert self-control to continue squeezing the handles despite the uncomfortable condition. The longer participants kept on squeezing, the better their self-control performance on this task.

Creative problem solving task. This task was used as an operationalization of start self-control. We created this task using elements from tasks used by Vohs et al. (2008) and by Guinote (2007b). Participants were given time to study for an ostensibly upcoming creative problem solving abilities test that was framed as a predictor of many desirable life outcomes. Additionally, participants were told of past research showing that being familiar with the test materials significantly improved performance on the test and that a practice period of 15 min had proven to be sufficient. The experimenter announced that she would leave the room for 15 min and gave participants a sample item for practice. This alleged sample item was a geometrical form that looked like the contours of a building. Participants had to cross each wall only once in one continuous line. They were told that they could use as many copies as they needed to find the solution.

However, to make the task more difficult in terms of self-control, participants were also allowed to read magazines or surf the Internet (magazines and an iPad were on the table) if they did not wish to work on the sample item for the entire practice period. We used the number of attempts that participants made to solve the sample item as an indicator of task performance.

Focusing on the task at hand and getting (unpleasant) work done is part of our definition of start self-control. The creative problem solving task requires participants to start and keep working on a frustrating task, while distraction in the form of pleasant activities to occupy their time is nearby. Therefore, the participant must exert self-control to stay focused and continue working on the task.

d2 Test of Attention. This task was used as an operationalization of stop self-control. It is a timed test of selective attention/concentration, and it measures processing speed, rule compliance, and quality of performance in response to the discrimination of similar visual

stimuli. The test consists of 14 lines, each comprised of 47 characters (the letters *d* and *p*) with one to four dashes, for a total of 658 items. The participant must scan each line and cross out each d with two dashes. Participants had 4 minutes to work on as many characters as possible. We used the error rate (i.e., the ratio of total errors divided by the number of attempted items) as an indicator of task performance.

Stop self-control entails the suppression of impulses. On the d2 Test, participants should be tempted to cross out all ds irrespective of the number of accompanying dashes, because the visual distinction between ds and ps is far more obvious than the distinction between the target stimuli and all other kinds of characters. Self-control is required to ignore distractors and override the tendency to act naturally (cross out all ds).

Emotion regulation task. This task was used as an operationalization of stop self-control. Participants were seated in front of an iMac and asked to watch a video that contained three short films separated by a short break of 12 sec. To ensure that the effects were due to self-control rather than the particular emotional response, we used positive, negative, and boring stimuli. Based on the results of our pretest, we chose an *Ice Age* compilation (5:56 min), the trailer for *Amityville Horror* (2:12 min), and a documentary on traffic near Lucerne's main train station (3:48 min). Participants were told that the iMac was also filming their faces while they watched the video. They were instructed to watch the video and not show any emotions, so that another person watching the filming of their faces would not be able to guess which video they were watching.

Two raters blind to the experimental condition rated the emotional expressiveness of participant's faces on a 5-point scale (1 = absolutely non-expressive, 5 = very expressive). The interrater reliability (consistency definition) on the basis of mean ratings over three emotions per participant was ICC = .66. We used the mean rating of two raters across the three films as an indicator of task performance.

Keeping oneself from doing something one would *want* to do or one would *naturally* do is a defining part of stop self-control. In this emotion regulation task, videos prompted the participants to show emotions, but the participants were instructed not to do so. They had to exert self-control to override the natural tendency to spontaneously express their emotions.

Results

After data collection but prior to the analyses, we preregistered an analysis plan on the Open Science Framework (osf.io/u9xa2), specifying scale construction, decision rules, and planned confirmatory analyses.

Manipulation Checks

As expected, interviewers felt more powerful (n = 48, M = 3.68, SD = 0.73) during the interview role play than job applicants did (n = 47, M = 2.72, SD = 0.92), $t(93) = 5.62$,

$p < .001$, $d = 1.15$, 95% CI [0.71, 1.60]. This effect was robust insofar as for the second manipulation check, interviewers still felt more powerful ($M = 1.51$, $SD = 1.35$) than applicants ($M = 1.09$, $SD = 1.00$), $t(84.67) = 1.74$, $p = .043$, $d = 0.36$, 95% CI [-0.06, 0.78]. According to their own reports, interviewers invested less effort in the role play ($M = 2.25$, $SD = 1.26$) than applicants ($M = 2.94$, $SD = 1.42$), $t(93) = 2.49$, $p = .015$, and they were more satisfied with their performance ($M = 3.90$, $SD = 0.97$) than applicants ($M = 3.40$, $SD = 1.08$), $t(93) = 2.34$, $p = .022$. However, there was no difference in how well they liked the role play task, $t(93) = 1.05$, $p = .30$.

Main Analyses

Start self-control

We regressed the second handgrip measurement on the baseline measurement, saved the residuals, and used this residualized performance as a dependent variable in a t-test. Contrary to our hypothesis, interviewers (residualized handgrip performance in sec: $M = 2.1$, $SD = 76.74$) did not show more perseverance in the handgrip task than applicants (residualized handgrip performance in sec: $M = -2.1$, SD = 61.18), $t(93) = 0.30$, $p = .384$, $d = 0.06$, 95% CI [-0.35, 0.47]. In a similar vein, interviewers ($M = 10.42$, $SD = 4.34$) did not make more attempts to solve the creative task than applicants did ($M = 10.52$, $SD = 4.55$), $t(92) = 0.11$, $p = .455$, $d = 0.02$, 95% CI [-0.39, 0.44].

Stop self-control

Contrary to our hypothesis, interviewers ($M = .04$, $SD = 0.06$) and applicants ($M = .04$, $SD = 0.06$) did not differ in the error rate in the d2 Test of Attention, $t(93) = 0.16$, $p = .438$, $d = 0.02$, 95% CI [-0.44, 0.38]. Similarly, interviewers ($M = 1.47$, $SD = 0.61$) were not better at suppressing their emotions than applicants were ($M = 1.42$, $SD = 0.48$), $t(93) = 0.41$, $p = .680$, $d = 0.08$, 95% CI [-0.33, 0.50].

Discussion

Previous research has found that social power both benefits and harms self-control performance, but a theoretical explanation for this paradox was lacking. We suggested that social power would increase start self-control, which is necessary for initiating and maintaining behavior, but would decrease stop self-control, which is necessary for suppressing behavior or refraining from acting impulsively. This distinction helped us organize the contradictory findings in the published literature on power and self-control (see Table 2). However, the main goal of this study was to use the start/stop distinction to test a priori hypotheses regarding the differential effects of power on start self-control vs. stop self-control. To this end, we used a role play manipulation that allowed us to experimentally create large differences in participants' feeling of power or powerlessness. We used four

tasks to represent the two self-control components. Participants completed the d2 Test of Attention and an emotion regulation task (both considered to be inhibitory) as well as a handgrip task and a creative problem solving task (both considered to be initiatory).

The results of this pre-registered experiment are non-significant with regard to all four self-control tasks. More precisely, high power and low power participants do not differ in their endurance in the handgrip task, the number of attempts made to solve the problem-solving task, the number of errors made on the d2, and externally rated success in suppressing their emotions. We would like to preface the interpretation and discussion of these results by saying that effects of power on self-control may well exist, but they are unlikely to be large. In fact, the power analysis underlying our experiment allows us to conclude that the effects are most likely smaller than $d = .64$. This is the correct interpretation of non-significant effects in this study and should be kept in mind when we talk in the following more categorically about the presence or absence of effects.

Thus, the results of the present experiment are inconsistent with our predictions regarding the differential effects of social power on start/stop self-control. However, rather than merely disconfirming the direction of the effects postulated by the start/stop distinction (i.e., whether power decreases or increases self-control), our results more generally call into question the existence of the effects of power and self-control as postulated by the social distance theory of power (Magee & Smith, 2013) and the approach/inhibition theory of power (Keltner et al., 2003).

Possible Explanations for the Null Findings

An effect depends on the outcome variable, the recipients of a treatment, the setting, the time, and the treatment (Reichardt, 2006). Reasons for the absence of an effect might be found in one or more of these five factors.

First, with regard to the outcome variable, we must state that every empirical study involves auxiliary assumptions regarding the operationalization of the outcome. It is possible that the chosen self-control measures are not good indicators of the two postulated self-control dimensions. For instance, the d2 Test of Attention might be considered to be an operationalization of start self-control, because the task – to focus on target stimuli and ignore distractors – is highly similar to the Stroop task, for example, which we classified as a start self-control measure in our literature review. However, even if our measures do not represent the initiatory and inhibitory component well, we used four operationalizations that are well established within the self-control and ego-depletion literature. Prior studies reported that these measures were sensitive to interindividual differences and experimental manipulations (e.g., Friese, Messner, & Schaffner, 2012; Guinote, 2007b; Muraven et al., 1998; Vohs, Baumeister, & Ciarocco, 2005). Accordingly, if self-control had been affected by our treatment, we would have expected to find variability in these measures as well.

Second, with regard to the recipients of the treatment (i.e., our participants), a possible alternative explanation is that we used a sample of female students, whereas most previous studies have relied on mixed gender samples. As stated in the introduction, we were interested in the participants' willingness to show self-control (not their ability), which we assumed could be altered by a power manipulation. Probably, highly conscientious participants would want to do their best in an experiment – irrespective of contextual factors such as manipulations; this would minimize variability in willingness to exercise self-control. Previous findings show that across different cultures, women score higher than men on Conscientiousness (Schmitt, Realo, Voracek, & Allik, 2008), and psychology students tend to be more conscientious than students majoring in other disciplines (e.g., Vedel, Thomsen, & Larsen, 2015). Indeed, our sample reported relatively high scores on conscientiousness (on a 7-point scale: M = 5.31, SD = 0.93). However, previous psychological research found interindividual differences in state self-control performance in predominantly female student samples (e.g., Martijn, et al., 2007; Vohs et al., 2008), so that this is unlikely to be the reason for our null findings.

Third, with regard to the setting, we have to note that due to administrative reasons the experimental sessions took place in two different rooms. However, we took great care to furnish and prepare the rooms as similarly as possible. Furthermore, it is very likely that our setting is highly similar to the setting in previous psychological research within a university context where studies detected differences in self-control measures. Therefore, we do not think that the setting is responsible for the absence of power effects.

Fourth, with regard to the time factor, participants completed the experiment at different times of the day. Although it is possible that self-control performance might vary over the course of the day, for example due to ego-depletion (Kouchaki & Smith, 2014), we expect that this potential time effect was averaged out, as we tested participants both in the mornings and afternoons. Time also refers to the time lag between treatment and measurement of the outcome variable. In this regard, it is reassuring to note that our manipulation check indicates that participants in the high power condition still felt more powerful at the end of the experiment than participants in the low power condition did. This suggests that it is unlikely that the effect of the interview role play was too short-lived to affect all of the outcome variables.

Fifth, with regard to the treatment, we have to acknowledge that this study is the first study on power and self-control to manipulate the feeling of power via a structural manipulation. Previous research in this domain adopted experiential and conceptual manipulations, such as episodic priming, conceptual priming, and not enacted role assignment (see Table 3.2 in the Tables section). Diverse power manipulations could – mediated by distinct processes – have different effects on the same dependent variable.

Experiential and conceptual manipulations are likely to activate the cognitive power network (see Tost, 2015), which might be responsible for the previous findings on the relationship between power and self-control. However, the role play manipulation that we used might have failed to activate this power network. Nevertheless, the results of our manipulation check support the conclusion that we succeeded in affecting the participant's experience of power. Hence, we doubt that the absence of effects on self-control performance is due to our power manipulation.

Strengths of this Study

This study has two notable strengths. First, this study employed a newly created power manipulation that contains important elements of previous role play manipulations. For example, the powerful participant has the mandate to direct the powerless participant and may evaluate the performance of the powerless participant with real consequences for the powerless participant. Besides, our manipulation has considerable mundane realism and fits the background of our participants, as most of them have probably already experienced comparable interview situations, either as candidates or interviewers or in both roles. Second, we see this mock interview as a suitable situation, because participants (mostly students and employed adults of the same age) in a prior study (see Chapter 2) often provided interview situation examples when asked to describe a situation in which they had or lacked power. Moreover, this manipulation creates a strong situation in which the role requirements are clear, and therefore, the participant's personality dominance should not have a strong effect (as may be possible in open situation role plays with a non-naturalistic task, such as in Schmid Mast, Jonas, & Hall, 2009, Study 1).

Furthermore, the statistical conclusion validity of this experiment is high, given the a priori power analysis and the preregistration of hypotheses and planned analyses. To reiterate our main conclusion, the non-significant results mean that the population effect is smaller than the one assumed in our power analysis, i.e., $d = .64$.

Conclusion

In a conceptual replication we observed effects that are inconsistent with the large effects of social power on self-control reported in the published literature. After considering several alternative explanations, the most plausible one is that the existing literature has overestimated these effects. This seems all the more plausible in the light of the results of the large reproducibility project (Open Science Collaboration, 2015). This study aimed to replicate more directly a great number of effects from top journals. Effect sizes of the replications were consistently smaller than the original, published effect size if the original effect size was greater than $d = .5$, indicating a potential publication bias. Given its practical importance, future research should further explore the relationship between social power and

self-control and try to identify possible moderators. Our newly developed role play power manipulation seems to be a promising starting point, in that it produces substantial differences in felt social power. With regard to the self-control operationalization, it might be worth investing further effort in validation of the start/stop distinction proposed here and sampling additional tasks that capture these aspects of self-control.

CHAPTER 4: Manipulating Social Power Within-Person

Abstract

Surprisingly, only very few studies have tested hypotheses about social power's effects in within-person designs, although power research could benefit from advantages of this approach such as increased statistical power and its more natural alignment with the everyday experience of most people. Our goal is to show what insights could be gained by studying power within-person. We propose to disaggregate power into its stable and dynamic components and discuss the roles of time, direction of change and individual differences in understanding the relationship between these components. Additionally, we present initial empirical evidence on the effect of power gain vs. power loss. Participants assumed first either a structurally high power or low power position in a newly developed role-play task and changed in the second round their roles and interaction partners. Subsequently to each role-play participants completed measures of risk-taking and interpersonal accuracy. Within the risk-taking measure we manipulated outcome responsibility between person that is whether the decision affected only the decision maker or both the decision maker and the interaction partner in the role-play. The order in which participants played the two power roles mattered for all outcomes. The power manipulation affected participant's feelings of power only when they gained structural power but not when they lost it. Similarly, risk-taking differed in the two order conditions and the preregistered hypothesis of reduced risk-taking for participants with outcome responsibility was not supported. Suggestions for further within-person power research are discussed.

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Introduction and Theoretical Background

So far, only very few studies have tested hypotheses about the effects of social power in within-person designs, be it experimental work (Goodwin, Gubin, Fiske, & Yzerbyt, 2000; Li, Wang, Yin, Mao, Zhu, & Huang, 2016; Sivanathan, Pillutla, & Murnighan, 2008; Weick, McCall, & Blascovich, 2017, Study 2) or field studies (Foulk, Lanaj, Tu, Erez, & Archambeau, 2018; Smith & Hofmann, 2016). This is surprising on both theoretical and methodological grounds because within-person designs have some advantages in comparison to between-person designs that have been predominantly used in power research (Charness, Gneezy, & Kuhn, 2012): First, the error variance associated with individual differences is reduced. In theory, individual differences should be controlled via random assignment of participants to conditions but in practice it is still possible that individual differences cluster in conditions. This problem can be circumvented by the use of within-person designs in which the participants are the same in the different conditions and accordingly, the conditions are identical with respect to individual differences. Second, within-designs offer a substantial boost in statistical power. Third, in many circumstances, within-designs are more naturally aligned with theoretical mindsets in that a researcher might want to find out how an individual reacts to different contexts and things instead of how two people react in two different contexts to two different things. From prior power research we only know how different people think, feel and behave when they find themselves in *either* high or low power conditions. What is missing is theory and empirical data on the same people being in *both* high and low power conditions, as it would correspond to the everyday reality of most people (Leikas, Lönnqvist, Verkasalo, & Nissinen, 2013; Schmid Mast, 2010).

This paper aims at preparing the ground for studying social power within person more systematically. We begin by disaggregating power into its stable and dynamic components. We discuss questions of theoretical interest about the relationship between these components that can only be answered by using within-designs. Finally, we address one of these questions empirically in testing the effects of a within-person structural power manipulation on psychological power and two outcomes, risk-taking and interpersonal accuracy.

Different Forms and Levels of Power

On every single day in our life we experience different forms and levels of power in various contexts – for instance as supervisors or employees at work, at home as parents with our children, as partners in our romantic relationships and as friends or colleagues in leisure time relationships. Depending on the respective relationship, a given individual has more asymmetrical and objectively demonstrable control over resources on which the interaction partner depends (i.e., structural power) in one situation, but controls less resources than the interaction partner in another situation (Keltner, Gruenfeld, & Anderson, 2003; Thibaut &

Kelley, 1959; Tost, 2015). The control over resources valued by the interaction partner is the basis for exerting influence over him/her. This implies that power is actually a property of a relationship and as such does not automatically generalize across contexts (Keltner, Van Kleef, Chen, & Kraus, 2008).

Consider for instance the example of Mary: She has two small children and works part-time as shop assistant in a clothes store. It is likely that Mary has a relatively high level of structural power vis-à-vis her children because she as parent provides resources such as food, safety, and comfort that are critical for her children. However, in her job she has a rather low level of structural power vis-à-vis her boss, as her boss decides how many shifts she works, when she can take two weeks off to go on holiday with her family and if she gets the full end-of-year bonus or not.

Still, power is not only an objectively assessable characteristic of an interpersonal relationship but can also be psychological – the subjective assessment of one's own ability to influence others in a given social situation (Anderson, John, & Keltner, 2012). Many factors determine this assessment such as personality (extraversion, dominance, e.g., Anderson, Spataro, & Flynn, 2008), competence (e.g., Darioly & Schmid Mast, 2011), and structural power (e.g., Schmid Mast, 2010). One would intuitively assume that high structural power goes along with high levels of psychological power. Experimental power research builds on this logic when using structural manipulations such as role-plays or manipulations of the control over resources in ultimatum or dictator games (Galinsky, Rucker, & Magee, 2015).

However, the relationship between structural and psychological power does not necessarily have to be positive as more applied research in family or organization contexts shows (e.g., van Dijke, de Cremer, Langendijk, & Anderson, 2018). For instance, although parents are in a high structural power position vis-à-vis their children, their self-reported level of psychological power does not necessarily have to be high, too (e.g., Bugental & Lewis, 1999). In an experience sampling study that collected reports about potentially hierarchical interactions in work, family and leisure time contexts, structural power predicted feelings of power imperfectly (Smith & Hofmann, 2016). Interestingly, participants in a structural low-power position were more likely to report relatively high levels psychological power, than participants in a structural high-power position were to report relatively low levels of psychological of power.

Studying Power Within Person

As the preceding section illustrates, every individual changes his/her level of structural power when changing from context to context and relationship to relationship probably quite frequently along the course of a day. But – given that we have also seen that there is no one-to-one relationship between structural and psychological power – we know far less about changes (or stability?) in psychological power. This calls for more research as psychological

power has been seen as the main driver responsible for effects reported in social psychological (laboratory) power research (Galinsky et al., 2015).

Initial empirical evidence showed that psychological power is specific to particular relationships, but that it is also moderately consistent across relationships (Anderson et al., 2012). Accordingly, psychological power in situation X might be a function of (1) a trait-like aggregate assessment of psychological power i.e., trait power and, (2) situational influences that are constituted by (a) situation-specific structural power and (b) other situational ("unsystematic") variation due to for instance the interpretation of structural power in this specific situation, mood, health condition, sense of achievement and so on (Fleeson & Noftle, 2008; see Figure 4.1). This conception sets the stage for many interesting theoretical questions about the dynamics of (psychological) power.

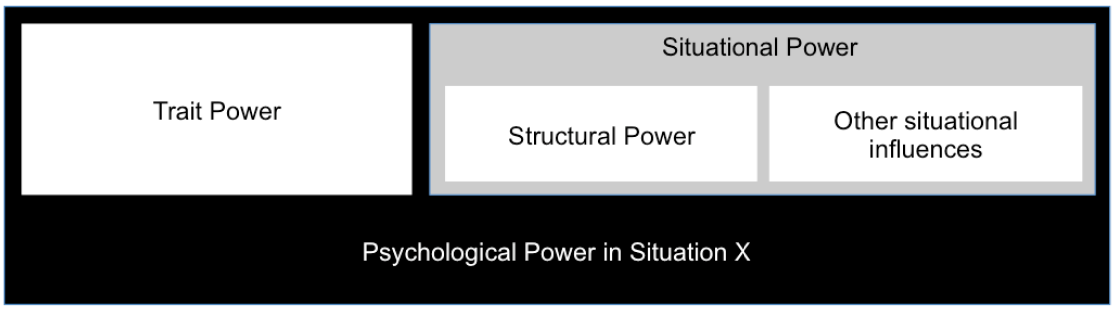

Figure 4.1. Conceptual model of determinants of psychological power

First question: The impact of time

What is the role of time in the interplay between trait power, structural power and psychological power? Unfortunately, time has not played a significant role in psychological theory building (neither within power research nor in psychological research in general), although time impacts *what* constructs are and *how* and *why* they are related (George & Jones, 2000). For instance, on a very general level, how in time is situational power aggregated to form trait power? Time is an ongoing stream of subjective experiences that becomes meaningful in the moment individuals partition this stream into separate episodes they reflect upon and interpret. It is possible that trait power is the aggregate of all prior experiences of power – but it might also be that only the power related experiences in the current phase of life such as adolescence, early adulthood or midlife decisively shape trait power.

Furthermore, more granular questions on the timing of power experiences in specific situations arise. Two example questions are illustrated by Figure 4.2, which is meant to represent the experiences of a given individual. Figure 4.2 shows a coordinate system with time on the x-axis and power in its different forms on the y-axis. The solid line indicates this individual's level of trait power that is seen as the constant power baseline over the depicted

observation period. Within this period, the individual had a structurally high power role (e.g., interviewed a possible future intern) and a structurally low power role (e.g., presented the quarterly figures to her boss). The dotted line depicts a possible trajectory of psychological power the individual might experience within the observation period. It is assumed that the experience of different levels of structural power shapes this trajectory. Two example questions concern the immediacy of changes in psychological power as a consequence of changes in structural power. For instance, how long does an individual need to be exposed to structural power or experimental manipulations of psychological power in order for this experience to translate into psychological power (see parameter a)? Auxiliary analyses of power priming data suggest that a minimum of time in which individuals engage with power might be necessary for effects to develop (see Chapter 2). How long does it take until an individual can disengage from a specific level of psychological power (see parameter b)? As power experiments mostly contain a power manipulation *directly* followed by a manipulation check and only one dependent measure there is no empirical data that sheds light on this issue.

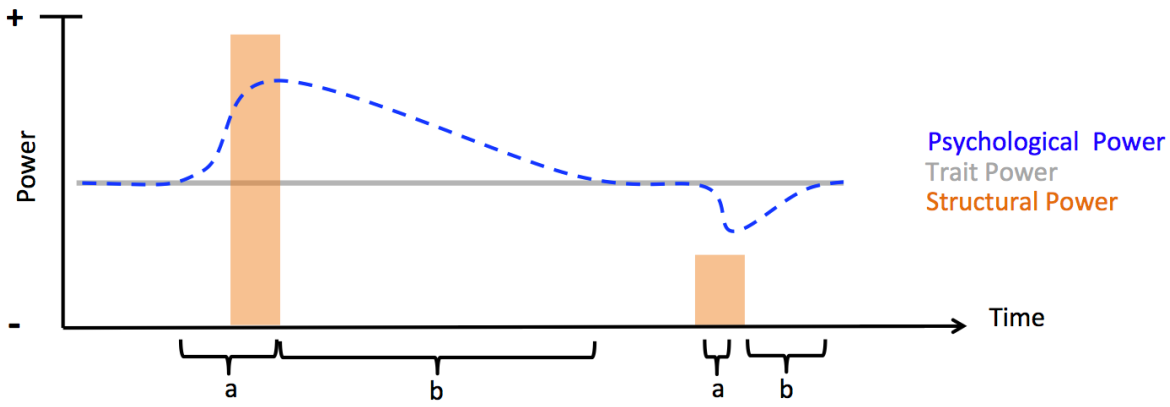

Figure 4.2. Temporal fluctuations in individual X's psychological power

Second question: The impact of direction of change

Figure 4.3 illustrates considerations on the order in which varying levels of structural power are experienced. Similarly to Figure 4.2, Figure 4.3 is meant to depict a given individual's power experiences in a coordinate system with time on the x-axis and power in its different forms on the y-axis. The solid line indicates this individual's stable level of trait power. This time, the individual had first a structurally high power role (e.g., discussed his assistant's salary with him), then a structurally low power role (e.g., explained to his boss why an important customer order has not been completed in time) and then again a structurally high power role (e.g., settled a dispute between two subordinates on the distribution of tasks). The dotted line depicts again a possible trajectory of psychological power.

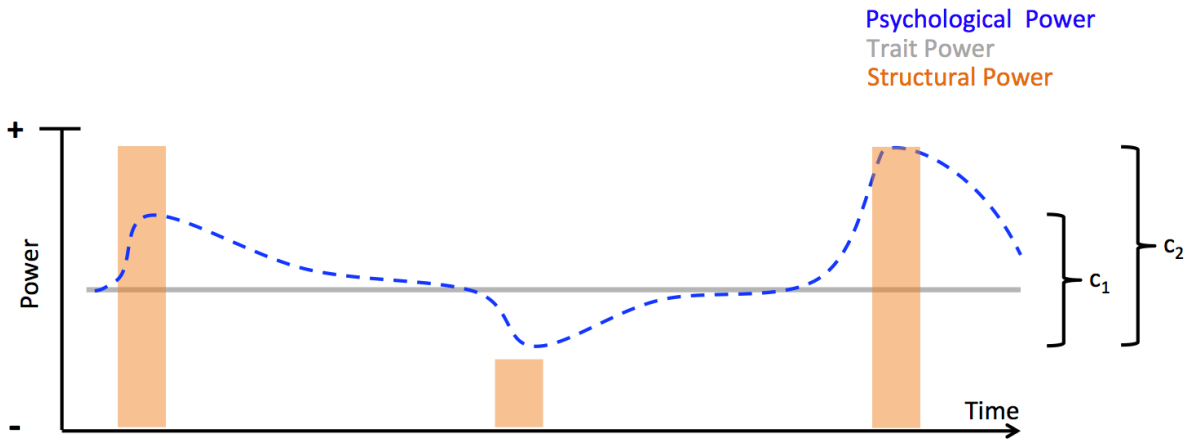

Figure 4.3. Effects of power loss and power gain on psychological power

Does the direction of change, i.e., power loss (see parameter c_1) or power gain (see parameter c_2), matter? Initial empirical evidence points to an affirmative answer: In the experiments by Sivanathan and colleagues (2008) participants moved from ultimatum to dictator game (i.e., power gain) or from dictator game to ultimatum game (i.e., power loss). In two of the four experiments there were also two control conditions included, in which participants played either two times the ultimatum game or two times the dictator game. In terms of participants' demands, the results consistently showed that people reacted more strongly to an increase in power (i.e., substantial increase in demands) than to a loss in power (i.e., no strong decrease in demands). Unfortunately, the authors did not report a manipulation check for psychological power, so we do not know how the changes in structural power affected participants' psychological power.

This issue is closely related to the question where "baseline psychological power" is located. This is (also) an important but unresolved question in the classical between-person power research. The mostly implicit assumption is that participants have as default an intermediate level of psychological power based on which they can be moved in states of high or low power. However, as three-cell designs including control conditions are rather the exception than the rule, it is impossible to draw a final conclusion whether power or powerlessness is the driver of effects (Moskowitz, 2004; du Plessis, Schaerer, Yap, & Thau, 2016).

It is likely that (healthy and typical) participants in power experiments would report rather high levels of psychological power as baseline. From a theoretical point of view, feeling in control is a fundamental human need (for reviews see Mineka & Hendersen, 1985; Pittman & Heller, 1987). Having a sense of control is so important that it even drives individuals to see non-existent patterns in their environment (Whitson & Galinsky, 2008) or to anthropomorphize nonhuman agents because the readily accessible concept of "human" can serve as a useful explanatory source for understanding, controlling, and predicting another agent's behavior (e.g., Epley, Waytz, Akalis, & Cacioppo, 2008). Indeed, with regard to

empirical findings, student participants in studies by Anderson and colleagues (2012) reported for the majority of studied contexts (i.e., participants in relationships with their parents, friends, current dating partners, supervisors at work, teaching assistant from a current class, sorority and dormitory members, fellow MBA-students in an experiment) levels of psychological power that are significantly higher than the scale midpoint. In a similar vein, non-student participants in an experience sampling study by Smith and Hofmann (2016) reported levels of psychological power above the scale midpoint.

In answering the question whether the direction of power changes matters, the point probably is how individuals belonging to different populations internally interpret the psychological power scale. Non-student participants presumably have a quite good and "accurate" understanding of the scale point's meaning as they have already experienced a wide range of levels of (structural) power. In contrast, participants from student populations (as commonly studied in power research) might interpret the same scale quite differently. As detailed above, they might initially indicate rather high levels of psychological power as baseline. However, additionally giving them structural power - i.e., allowing them to see what power *really* is: making things happen they want to happen, influencing others, controlling resources - might invite them to reevaluate their interpretation of power. Hence, they would not have much room to indicate a substantially higher value on a psychological power scale, but this reevaluation could still have strong effects on dependent variables (cf. see above Sivanathan et al., 2008). In contrast, power loss in experimental settings would mean that student participants do or report what they probably experience as normal – carrying out instructions in role plays (as they do in relationships with their parents, professors or supervisors) or thinking about situations in which they did so in episodic priming tasks. Again, the value on the psychological power scale would probably not change dramatically and no strong effects on dependent measures would be expected. Taken together, power gain might produce stronger effects than power loss in student participants. However, this does not have to be the case for non-student participants. Due to the underrepresentation of non-student samples in power research this remains an open question.

Third question: The impact of individual differences in trait power

Does everybody respond equally strongly to changes in structural power? Figure 4.4 illustrates the negative answer to this question: the figure shows in a coordinate system with time on the x-axis and power on the y-axis the experiences of two individuals, individual D high in trait power and individual E low in trait power. Both individuals find themselves in a structurally high power and a structurally low power position. The dotted line depicts two differing trajectories in response to the two different levels of structural power. It might be that placing people high in trait power such as individual D in a position of high structural power does not affect their psychological power strongly (see parameter d_1). Conversely, placing

someone low in trait power such as individual E in the same position of structural power increases his/her psychological power profoundly (see parameter e_1). Vice versa, people high in trait power such as individual D might react more strongly to being in positions of low structural power (see parameter d_2) as compared to people low in trait power such as individual E react to being in positions of low structural power (see parameter e_2). This again might be a matter of personal default: the larger the difference between trait power and structural power, the stronger the effects on psychological power might be.

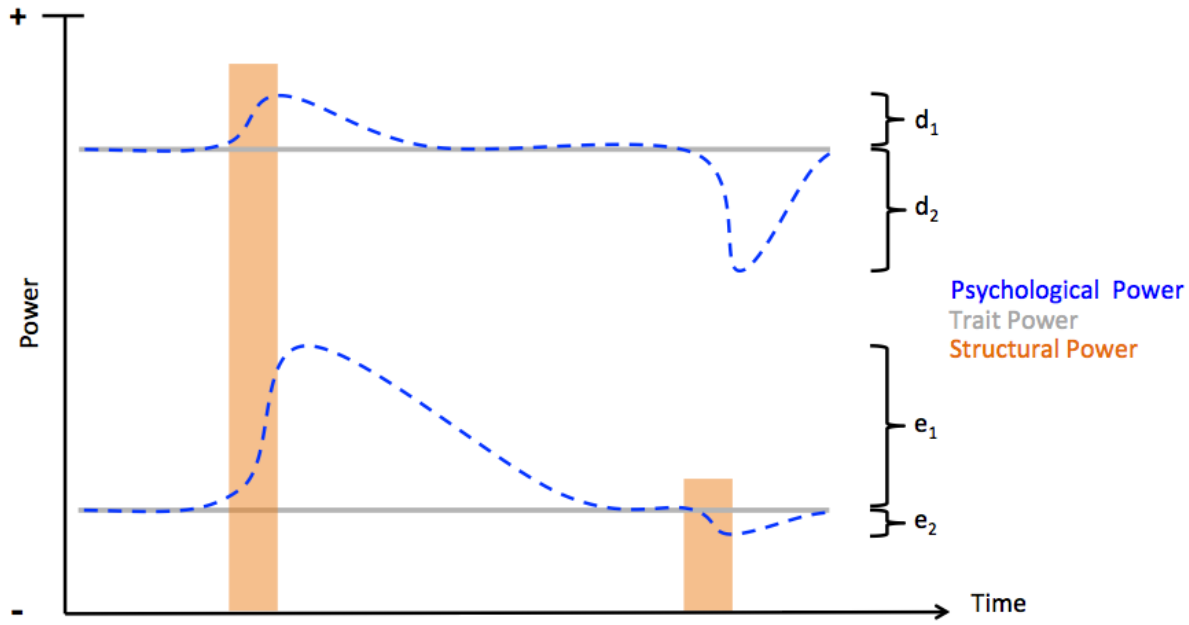

Figure 4.4. Fluctuations in psychological power for individuals D and E

The current work

All these (and more) questions can only be answered on the basis of within-person designs. In the current work, we attempted to empirically tackle the question whether the direction of change matters as it is the easiest to answer given that only two data points per participant are needed to operationalize power gain and power loss. In the study reported here we manipulated power within-person using a newly developed role-play task. Participants assumed first either a structurally high power (power loss condition) or low power position (power gain condition). Subsequently to the role-plays they indicated their levels of psychological power and as such our study extends the findings by Sivanathan and colleagues (2008).

Aside from investigating the within-person relationship between structural power and (trait and situational) psychological power, we also explored whether previously studied dependent variables within power research are not only affected by *between*-person manipulations, but also susceptible to *within*-person manipulations. We chose two very

different outcomes, risk-taking and interpersonal accuracy. We did so because of the following two reasons: on the one hand previous research has suggested that moderators might qualify the effect of power on these two variables and we attempt to test one of them. On the other hand we wanted to cover the full range of possible outcomes from agentic (i.e., behaviors that focus on self-assertion and independence such as risk-taking) to communal (i.e., behaviors related to social attention and interpersonal responsiveness such as interpersonal accuracy), because of the still unsettled discussion whether a specific power manipulation such as episodic priming or role-play would have different effects or effects at all on the same (agentic or communal) outcome (Tost, 2015). Interpersonal accuracy has been studied previously using role-play manipulations (e.g., Anderson & Berdahl, 2002; Gonzaga, Keltner, & Ward, 2008; Schmid Mast, Jonas, & Hall, 2009), whereas risk-taking has not.

Power and Risk-Taking

Risk-taking involves making a decision or taking action that could lead to negative consequences such as for instance physical harm, financial loss or disapproval by the social environment (Byrnes, Miller, & Schafer, 1999) but might also present an opportunity for a rewarding outcome. According to two prominent power theories, the powerful should be prone to risk-taking. The approach/inhibition theory (AIT, Keltner et al., 2003) posits that having power activates the reward-sensitive behavioral approach system, which in turn leads to more risk-taking. The social distance theory (SDT, Magee & Smith, 2013) assumes that having power induces an abstract mental representation (i.e., higher level construal) that has been shown to increase risk-taking within the gain domain (Lermer, Streicher, Sachs, Raue, & Frey, 2015; Raue, Streicher, Lermer, & Frey, 2015).

Numerous studies have examined the hypothesis of increased risk-taking propensity in the powerful with generally supportive results (Anderson & Galinsky, 2006; Carney, Cuddy, Yap, 2010; Fast, Gruenfeld, Sivanathan, & Galinsky, 2009; Fast, Sivanathan, Mayer, & Galinsky, 2012; Hiemer & Abele, 2012; Jordan, Sivanathan, & Galinsky, 2011; Maner, Gailliot, Butz, & Peruche, 2007; Ronay & Von Hippel, 2010; but see Ranehill, Dreber, Johannesson, Leiberg, Sul, & Weber, 2015; Comprehensive Results in Social Psychology special issue on power poses). These studies have in common that the (risky) decision only had consequences for the decision-maker him-/herself. However, this focus is at odds with the fact that the powerful in the real world usually take risks not only for themselves, but also for other people. For instance, decisions made by CEOs usually concern employees, shareholders and various stakeholders, decisions made by a head of state concern the own and/or other nations in the present or future and so on.

We want to address this gap by introducing a possible moderator of the effects of power on (risky) decision-making: outcome responsibility, that is whether risky decisions

affect only the self or both the self and another person within the power relationship. Indeed, prior risk-taking research has shown that when people are responsible for other's outcomes, they are less willing to take risks (Charness & Jackson, 2009; Bolton, Ockenfels, & Stauf, 2015; Pahlke, Strasser, & Vieider, 2015, Study 1; Wang, Kuang, Tang, Gao, Chen, & Qin Chan, 2018). Based on these arguments, we tested the preregistered hypothesis that the factors Power and Outcome Responsibility interactively affect risk-taking, such that the positive effect of power on risk-taking should be smaller when the decision-maker had outcome responsibility as opposed to when he did not (see osf.io/yxf6e).

Power and Interpersonal Accuracy

Interpersonal accuracy is the degree to which an individual is able to correctly assess characteristics of an interaction partner such as affective states, thoughts, and nonverbal behavior. The approach/inhibition theory (AIT, Keltner et al., 2003) and the social distance theory (SDT, Magee & Smith, 2013) agree on a negative relationship between power and interpersonal accuracy as default, albeit explaining this outcome differently, either by increased automaticity of social cognition (AIT) or perceived social distance (SDT).

So far, many studies have shown that having or lacking power affects interpersonal accuracy when assessing a target. However, it is not obvious *how*, as there is evidence in support of both the ideas that power improves interpersonal accuracy (e.g., Anderson & Berdahl, 2002, Study 2; Kunstman & Maner, 2011, Study 4; Schmid Mast et al., Study 1) and power impairs it (e.g., Galinsky, Magee, Inesi, & Gruenfeld, 2006; Gonzaga et al., 2008; Snodgrass, 1985). Several explanations have been proposed to make sense of these contradictory (laboratory) findings, namely based on characteristics of the power operationalization (Hall, Schmid Mast, & Latu, 2015; Tost, 2015) or characteristics of the accuracy measure (Bombari, Schmid Mast, Brosch, & Sander, 2013; Hall et al., 2015).

For instance, the meta-analysis by Hall and colleagues (2015) examined how various operationalizations of verticality (pre-existing vertical position, personality dominance, SES, experimental manipulation) are related to various ways to measure accuracy (inference vs. recall, stimulus material is either a standardized test vs. a real interaction partner). An important distinction is made between *inferential accuracy* which requires making inferences about other people's states, traits, attitudes, or personal attributes as conveyed through their nonverbal cues or a combination of their nonverbal and verbal cues, and *recall accuracy* which involves noticing and recalling others' (non-)verbal behavior (e.g., appearance, frequency of smiling or nodding). Importantly, inferential accuracy is dependent on the expressive clarity of the interaction partner (i.e., does the interaction partner actually show feelings that could be interpreted?), whereas recall accuracy is based on factual information that cannot be sent with varying degrees of clarity.

Interestingly, the meta-analysis by Hall and colleagues (2015) showed that in studies in

which one person perceives another person, people in experimentally assigned low power positions were more accurate than people in experimentally assigned high power positions when they had to make accurate interpersonal *inferences.* In contrast, when the task was to *recall* things about targets, there was no difference between people in experimentally assigned high and low power positions. In our study, we included both inferential and recall accuracy. Despite the meta-analytic null finding for recall accuracy, we see value in including it in order to have at least one measure that is not sensitive to the possible confound of differing levels of expressive clarity. Given the possibility that the five studies on which the null finding is based had a between-person design[6], we might still be able to find differences in a statistically more powerful within-person design.

Taken together, it is for our purposes not urgently relevant whether power increases or decreases interpersonal accuracy, what matters is the fact that in past research power has been shown to reliably affect interpersonal accuracy. Accordingly, we explore whether interpersonal accuracy is affected by power in a within-person design, and consider it to be a result supporting our hypothesis if states of low and high power produce significant differences in accuracy.

Method

The present research was done in accordance with the checklist issued by the responsible ethics committee of the Faculty of Philosophy, University of Zurich, meaning that no formal approval was needed. This research respects the Ethical Principles of Psychologists and Code of Conduct by the American Psychological Association (APA) as well as the Ethics Guidelines for Psychologists by the Swiss Psychological Society.

Power Analysis

A power analysis for our main hypothesis test suggested that a sample size of $n = 74$ affords 80% statistical power to detect an effect of at least $d = .50$, one-sided test, alpha error 10%. Due to our experimental design (group testing), the number of participants had to be a multiple of 4, i.e., 76 participants. In order to be sure to have a sufficient number of usable cases, we intended to collect at least 80 participants.

Sample

We recruited 82 men in the age range 18 to 59 years ($M = 27.12$, $SD = 9.19$; 63 students) from the pool of students and interested community members at the University of Zurich and the Swiss Federal Institute of Technology Zurich to take part in a study on decision behavior. We decided in favor of an all-male sample because prior research suggested that men are in general less risk averse (Byrnes et al., 1999; Cross, Copping, &

[6] Only two of the five included studies are publicly available. These two (Hall, Carter, & Horgan, 2001; Hall, Murphy, & Carney, 2006) used between-person designs.

Campbell, 2011) and we wanted to increase the probability to have a strong positive effect of power on risk-taking that we could reduce by our manipulation of outcome responsibility. Participants were paid either a fixed compensation of 15 Swiss francs (about 15 U.S. dollars) or received partial course credit. In addition, the compensation had a variable component that participants determined themselves via the decisions they made in the lotteries used to measure risk-taking (more information in the measures section). Participants could augment their compensation by between 0.15 to 20 Swiss Francs. The mean variable amount paid was 5.64 Swiss Francs. Due to technical problems with the administration of the risk-taking measure, 4 participants had to be excluded from all analyses, resulting in a final sample size of 78 men.

Procedure

Before coming to the laboratory, participants were asked to complete an online questionnaire that contained questions on demographics and potential moderator variables such as habitual decision-making style (Rational and Intuitive Decision Styles Scale, Hamilton, Shih, & Mohammed, 2016; Cognitive Reflective Test, Frederick, 2005), habitual risk-taking (DOSPERT, Blais & Weber, 2006; GESIS one item measure), promotion / prevention focus (items taken from Lockwood, Jordan, & Kunda, 2002), trait self-control (items taken from de Boer, van Hooft, & Bakker, 2011), explicit power motive (items taken from Cassidy & Lynn, 1989), trait power (Anderson et al., 2012), and social value orientation (Murphy, Ackermann, & Handgraaf, 2011).

At least 24 hours later, participants were invited to a laboratory experiment in which we manipulated two factors: social power as within-factor and outcome responsibility, namely the beneficiary of the decision, as between-factor. The power manipulation encompassed two role-plays with two different interaction partners: each participant experienced once the high power and once the low power role. Accordingly, participants were invited in groups of four.

Participants were welcomed and gave their written consent. Then, participants were randomly assigned to the two different roles that result from crossing power position and order: (1) high power first, low power second (power loss condition), and (2) low power first, high power second (power gain condition). An experimenter accompanied one dyad consisting of a high power and a low power individual to an adjacent room, the other dyad stayed with another experimenter in the first room. Both dyads were instructed to follow the same procedure.

Participants received written instructions that explained the rules of the role-play and the task of solving tangram puzzles (see also below power manipulation section). The dyads worked on this task for 10 minutes sitting on a large table in front of one another with the parts of the puzzle between them. Then, participants in the high power role evaluated the

participants in the low power role.

Subsequently, participants were reseated in front of computers facing each other and individually completed the risk taking measure. Depending on the outcome responsibility condition, participants made either decisions that affected their own variable compensation, or also the variable compensation of their interaction partner in the role-play analogously. Then, participants were asked to answer questions on their impressions of the joint work in the role-play and their interaction partner that were used to determine different kinds of interpersonal accuracy.

Once both dyads had finished the first part, participants in the power-loss-condition changed the room to meet the yet unknown participant in the opposing role. Participants were informed that this second round of the experiment would be highly similar to the first round, but that they should not try to remember their decisions in the first round and to only base their behavior and answers on the experiences made in this new situation. Moreover, they were asked not to talk about their experiences from the first round. The course of action in the second round was identical to the first round. In both rounds, participants were filmed while completing the tangram-solving task used as power manipulation. These videotapes were later coded by several raters and mainly served as one possible operationalization of interpersonal accuracy. As outcome responsibility was manipulated as between-person factor, participants who decided only for themselves in the first round did so also in the second round and vice versa for those who decided for the dyad. In the end, participants received their fixed and variable compensation, were debriefed and thanked.

Manipulations

Power manipulation

In both roles, participants read that they should imagine taking part in an internal assessment center organized to identify career development potential of employees. The goal of this ostensible assessment center task was the observation of behavior vital for cooperation. Within the role-play, the participant's task was to solve tangram puzzles. The tangram is an ancient Chinese jigsaw puzzle consisting of seven tiles that can be combined to form different shapes. Each participant dyad received in addition to the written instructions concerning their roles an envelope with the seven tiles and one of two tangram sets, i.e., sheets that showed 10 shapes each (only silhouettes). In addition, the high power participant received the solutions of the respective tangram set, i.e., a view on the shapes that indicated the position of the seven tiles. There were two versions of tangram sets, so that one participant worked on different sets when in the high power or low power role. The two tangram sets did not differ in difficulty as indicated by comparable numbers of solved puzzles

in the high power role (set 1: M = 4.53, SD = 1.87; set 2: M = 4.71, SD = 1.98), $t(76) = 0.43$, $p = .67$. The order of tangram sets was counterbalanced across all experimental sessions.

The high-power participant played the role of the *decision maker*. The decision maker was described as being in charge of the decisions how and in which order to solve the shapes from the tangram sets, how to structure the building process, and he also had to manage time as well as to evaluate the low-power participant at the end. Although the high-power participant had the solution, he was not allowed to show it to the interaction partner but he was instructed to only provide hints if necessary.

The low-power participant, the *assistant*, had to follow the instructions given by the high-power participant, and had to build the chosen shapes. Furthermore, he was informed that he could make suggestions but that the decision maker would make the final decision. Each role instruction contained also the basic information about the other role. Participants read that they would have ten minutes time for this task and that although they should try to solve as many shapes as possible, the focus should be on cooperation.

Subsequently to the tangram solving, the high power participant evaluated the low power participant. On 6-point-scales, high power participants indicated their satisfaction with the performance and work motivation of low power participants, rated the overall impression of their interaction partners und made a recommendation whether their interaction partners should be invited to enter the next round of the imagined assessment center (higher values meant greater satisfaction). Cronbach's alpha for these four evaluative items was .83. It made no difference for the evaluation whether participants were first in the high power role (M = 4.84, SD = 0.77) or second in the high power role (M = 4.88, SD = 0.88), $t(76) = 0.21$, $p = .83$, $d = 0.05$.

Manipulation of outcome responsibility

Participants made the choices between lotteries either for themselves only or for themselves and their interaction partner. In case participants made a decision involving another person, they were informed that their decision was anonymous, i.e., no participant would know how his variable compensation was composed.

Measures

Risk taking

We operationalized risk (in line with prior research within the gain domain, e.g., Adam, Kroll, & Teubner, 2014; Bolton et al., 2015; Eijkelenboom & Vostroknutov, 2016) adapting a method presented by Holt and Laury (2002). Participants were confronted twice in the experiment with two blocks of nine binary choices between two lotteries with different variabilities in outcomes. While the potential payoffs stay the same throughout one block of

choices, the probability of receiving the higher payoff increases, rendering the lottery with higher variability progressively more attractive.

For instance, in one block the safe lottery consisted of a decision between 3.80 CHF and 3.05 CHF in varying probabilities versus a risky lottery with decisions between 9.95 CHF and 0.20 CHF in corresponding probabilities. In order to limit memory effects, we chose the probabilities not in increments of 10 (in contrast to prior research). Namely, we chose 9% vs. 91%, 18% vs. 82%, 36% vs. 64%, 45% vs. 55%, 54%vs. 46%, 63% vs. 37%, 72% vs. 28%, 81% vs. 19%, and 90% vs. 10%. The amounts in the other three safe lotteries were 3.90 CHF vs. 3.10 CHF, 3.75 CHF vs. 3.00 CHF and 3.95 CHF vs. 3.15 CHF. The amounts in the other three risky lotteries were 10.05 CHF vs. 0.25 CHF, 9.90 CHF vs. 0.15 CHF and 10.10 CHF vs. 0.30 CHF. The four lottery blocks were presented in four different orders so that each block was once the first, once the second, once the third and once the fourth. We presented the nine choices per block in random order. Participants were informed that one of their choices in the four blocks would randomly be determined to be played and that they would receive the additional amount as variable part of their compensation. Following Holt and Laury (2002), we used the number of choices of the less variable lottery as a measure of risk aversion.

From the four blocks, only two were used in the analyses presented here, namely the ones in which participants made choices either for themselves only or for themselves and their interaction partner. The remaining two blocks that were not used here always referred to choices that would affect participants themselves and an unknown future participant.

Interpersonal accuracy

To operationalize interpersonal accuracy we used different approaches, as there is not *the* one way to do so. These approaches require different ratings from different sources: In a given role-play interaction, individual A and individual B work together. Both of them indicate subsequently to the interaction their own thoughts (5 items) and feelings (19 items) as well as what they think that their interaction partner thought and felt in the interaction. Example items for measuring thoughts were “[I am/ My interaction partner] is contented with our way to solve the task” and “[I /My interaction partner] would want to work again together with [my interaction partner/me]”. With regard to feelings, participants indicated how e.g., “relaxed”, “attentive”, and “confident” they and their interaction partner felt. The items concerning thoughts and feelings were answered on scales ranging from 1 (not at all) to 7 (completely). Participants also rated the extent to which their interaction partner showed different behaviors such as speaking, gazing, smiling, nodding self-touching and gesturing on a scale ranging from 1 (never) to 7 (very frequently). Additionally, these behavioral indicators were

rated by external raters who were as blind as possible to the hypotheses[7] (timed: speaking time and gazing; frequencies: smiling, nodding, gesturing and self-touching).

Self-rating (i.e., how does my interaction partner rate himself), other-by-self-rating (i.e., how do I rate my interaction partner) and external rating (i.e., how does a coder rate my interaction partner) allow for determining two indicators of interpersonal accuracy. First, the agreement between other-by-self-rating and self-rating with regard to thoughts and feelings describes *inferential* accuracy (cf. Ickes, Stinson, Bissonnette, & Garcia, 1990). Second, the agreement between the other-by-self-rating of behavior and the corresponding external rating describes *recall* accuracy.

Inferential accuracy. Inferential accuracy can be determined in two ways. On the one hand for a given participant the other-by-self-rating and the self-rating of the interaction partner can be correlated. This correlation is positive when the interaction partner's self-rating and the rating by the participant (other-by-self-rating) match. The higher the correlation, the higher is the inferential accuracy. On the other hand, the absolute differences between these two ratings can be averaged across the thoughts-items and the feelings-items. The smaller the difference score, the higher is the inferential accuracy. These two accuracy scores should be negatively correlated across participants.

Recall accuracy. Recall accuracy is determined following the procedure described in Hall, Murphy, and Schmid Mast (2006, 2007). Because the six behaviors are not all coded on the same metric (i.e., frequency vs. timing), each coded behavior is first z-scored across participants so that for each behavior each participant has a score that indicates how much of the behavior he engaged in relative to others in the experiment. Then, the other-by-self-rating regarding the six behaviors is correlated with his z-scores for the same behaviors (external rating), creating an individual profile correlation that represents an individual accuracy score. This correlation is positive when the participant ranked the partner's relative amounts of each behavior in a way matching the partner's actual behavior. The higher the correlation, the higher is the recall accuracy.

Behavioral coding

Both participants were coded at a time (frame rate per second: 0.7) and coders were instructed not to rewind the tape for more than 10 seconds. A subset of eight recorded interactions (this corresponds to 10 %) was chosen to establish interrater reliability. Three external, female coders, coders 1, 2 and 3, coded the six behaviors in these eight videotapes. Their ICCs were: .89 for speaking, .90 for eye contact, .95 for gestures, .89 for self-manipulation, .92 for smile and .73 for nodding. Coders 1 and 2 coded each half of the remaining videotapes.

[7] They were not familiar with the hypotheses regarding risk-taking and interpersonal accuracy, but they were probably able to figure out who was supposed to be the decision maker and who the assistant.

Manipulation checks

In order to assess the effects of our power manipulation, participants were asked how they felt during the interactions after each role-play. On a scale from 1 (not at all) to 7 (completely) participants indicated how much they agreed with several self-descriptive adjectives such as, “self-confident”, “powerful”, “dominant”, and “competent” [8]. These four items were averaged to build an indicator of felt power. For participants in the power-loss-condition, Cronbach’s alpha was α = .69. For participants in the power-gain-condition, Cronbach’s alpha was α = .78.

In order to determine whether our outcome responsibility manipulation was successful, we asked participants to indicate how responsible they felt on a scale ranging from 1 (not at all) to 7 (completely).

Results

Prior to data collection, we preregistered an analysis plan on the Open Science Framework (osf.io/yxf6e), specifying decision rules, and the planned confirmatory analyses. All analyses for manipulation checks and main hypotheses tests were done using the same mixed ANOVA with *Power* (decision maker, assistant) as within-subjects factor and *Order* (high power first/power-loss-condition, low power first/power-gain-condition) and *Outcome Responsibility* (yes, no) as between-subjects factors. We provide correlations for selected measured traits and measured experimental variables in Table 4.1 in the Appendix. It appears that trait power and situational power are moderately positively correlated; the same is true for the two risk-taking questionnaire measures. The numbers of safe choices constituting the experimental risk-taking measure in the first and second round were strongly correlated. In contrast, the variants of interpersonal accuracy measures are all non-significantly correlated across the two rounds (r = -.04 to r = .12). As expected, the difference scores and the profile correlation scores for inferential accuracy are significantly negatively correlated. Table 4.2 in the Appendix shows means and standard deviations of outcome variables by power-gain- and power-loss-condition.

Preregistered Manipulation Checks

Within factor power

At the very end of the experiment, participants had to indicate which roles they had played in the first and in the second role-play. All 78 participants remembered correctly the

[8] Participants also indicated how “unassertive” and “subordinate” they felt. Contrary to our scale construction proposal in the preanalysis plan these two items were excluded because they decreased the reliability of the self-constructed power manipulation check scale.

order in which they represented the decision maker and the assistant ($N_{\text{power loss + responsibility}}$ = 20, $N_{\text{power loss + no responsibility}}$ = 17, $N_{\text{power gain + responsibility}}$ = 20, $N_{\text{power gain + no responsibility}}$ = 21[9]).

The mixed ANOVA described above revealed a significant interaction between *Power* and *Order*, $F(1,74) = 25.17$, $p < .001$, $\eta_p^2 = 0.25$. In the power-loss-condition, participants did not differ in their self-rated power in the high power role and the low power role, $t(36) = 0.13$, $p = .895$, $d = 0.02$. However, in the power-gain-condition, participants reported feeling significantly more powerful in the high power role than in the low power role, $t(40) = 7.80$, $p < .001$, $d = 1.67$.

Between factor responsibility

We subjected the manipulation check item to the mixed ANOVA described above. We did not find the expected main effect of outcome responsibility, $F(1,74) = 0.11$, $p = .74$, $\eta_p^2 = 0.01$. However, we found a significant main effect of power, $F(1,74) = 27.11$, $p < .001$, $\eta_p^2 = 0.27$. Participants in the high power role felt more responsible ($M = 5.33$, $SD = 1.06$) than when they were in the low power role ($M = 4.13$, $SD = 1.58$), $t(77) = 5.33$, $p < .001$, $d = 0.60$. Given this unsuccessful manipulation check and anecdotal evidence reported by the experimenters that participants did not seem to have noticed this manipulation, we caution against attaching great importance to responsibility effects in this study.

Preregistered Main Analysis: Risk-Taking

We tested this hypothesis with the mixed ANOVA described above. Our main dependent variable was the number of safe choices. If our hypothesis was true, we should expect to find a significant interaction ($p < .10$, one-sided test) between *Power* and *Outcome Responsibility* with a probability of 80% (see power analysis above), indicating that the positive effect of *Power* on risk-taking is smaller in the condition with *Outcome Responsibility* as opposed to the condition without *Outcome Responsibility*. Additionally, it would be plausible to expect significant main effects of *Power* and/or *Outcome Responsibility*, but no other effects and interactions should be significant.

Unexpectedly, we only found a significant interaction effect of *Order* and *Outcome Responsibility*, $F(1,74) = 3.78$, $p = .06$, $\eta_p^2 = 0.05$ (note that in line with our power analysis we set the significance level to 10%). All other main effects and interactions were not significant.

In order to better understand the aforementioned significant interaction of *Order* and *Outcome Responsibility*, we further analyzed the power-gain-condition and the power-loss-condition separately. In the power-gain-condition there was a significant simple main effect of *Outcome Responsibility*, $F(1,39) = 4.99$, $p = .03$, $\eta_p^2 = 0.11$, such that participants who had outcome responsibility (HP: $M = 5.85$, $SD = 2.41$; LP: $M = 5.70$, $SD = 2.39$) made more safe

[9] Please note that the unequal cell sizes are the result of unforeseeable no-shows of participants. In this case one randomly determined role was played by an uninitiated confederate. However, as eight participants in total did not show up and the same confederate accordingly participated eight times, his data were excluded.

choices than participants without outcome responsibility (HP: M = 4.29, SD = 2.19; LP: M = 4.14, SD = 2.50). Neither the simple main effect of *Power* nor the interaction of *Power* and *Outcome Responsibility* were significant.

In the power-loss-condition we found a simple main effect of *Power*, $F(1,35) = 3.29$, $p = .08$, $\eta_p^2 = .09$, but no significant effect of *Outcome Responsibility*. The main effect of *Power* was qualified by an interaction of *Power* and *Responsibility,* $F(1,35) = 4.27$, $p = .05$, $\eta_p^2 = .11$, indicating that participants in the low power position without responsibility (M = 6.24, SD = 2.17) made more safe choices than the others ($LP_{Responsibility}$: M = 5.40, SD = 2.35; $HP_{Responsibility}$: M = 5.45, SD = 2.54; $HP_{NoResponsibility}$: M = 5.47, SD = 2.27). Taken together, these results show no support for our preregistered hypothesis expecting a reduced positive effect of power on risk-taking when the decision-maker had outcome reponsibility as opposed to when he does not.

Exploratory Analyses: Risk-Taking

The following observations might help to explain why the experimental data was not in line with our main hypothesis. In order for our main hypothesis (reduced risk taking in temporary powerful people with outcome responsibility) to receive support, the "default finding" of increased risk-taking propensity in response to having power should be true. Otherwise, it does not make sense to try to reduce the powerful's risk-taking. When comparing participants in low power and high power positions in their first round in the no-responsibility condition (that is best comparable to the previous studies examining how power affects risk-taking), we were not able to replicate the "default" finding": Participants in low power positions (M = 4.14, SD = 2.50) made less safe choices (i.e., took more risks) than participants in high power positions (M = 5.47, SD = 2.27), $t(36) = 1.70$, $p = .10$, $d = 0.55$.

Additionally, in this subsample and more importantly also in the whole sample, the problem is that participants made – irrespective of conditions and manipulations – so many safe choices that our responsibility manipulation did not have room to reduce risk-taking, $M_{\text{all conditions}} = 5.28$, SD = 2.41 compared to the theoretical mean of 4.50, $t(155) = 4.05$, $p < .001$ (remember: the answer scale ranges from 0 to 9). This general risk aversion might be due to our compensation strategy: Our participants received a fixed compensation of about 15 U.S. dollars for investing 1.25 to 1.5 hours of their time (questionnaire and laboratory experiment). Swiss student participants generally expect to receive this amount for a one-hour-lasting laboratory experiment, so they might have seen sticking to the safe choices as their only possibility to increase their compensation to come closer to their expected compensation.

Another reason why we failed to find support for our main hypotheses might be that participants in the power-loss-condition (M = 2.76, SD = 0.80) indicated significantly lower scores in one of two administered trait risk-taking measures compared to participants in the

power-gain-condition (*M* = 3.27, *SD* = 0.87), *t*(76) = 2.71, *p* = .008, *d* = .62, although participants were randomly assigned to the order conditions. These personality differences are mirrored in the deviations from the theoretical mean of the number of safe choices, *M* = 4.50. In the power-gain-condition, the participants' mean number of safe choices was not significantly different from the theoretical mean in both low power, *t*(40) = 1.02, *p* = 0.32, and high power positions, *t*(40) = 1.46, *p* = 0.15. In contrast, in the power-loss-condition, the participant's mean number of safe choices was significantly higher than the theoretical mean in both low power, *t*(36) = 3.43, *p* = .002, and high power positions, *t*(36) = 2.44, *p* = .02.

Given that our power manipulation was only partially successful, we also performed internal analysis (Wilson, Aronson & Carlsmith, 2010) and tested our main hypothesis using trait power and mean situational power as independent variable in correlational analyses. First, we regressed trait power (standardized), the experimentally assigned outcome responsibility (0 = no responsibility, 1 = responsibility) and their interaction term on the mean number of safe choices in the two power roles.

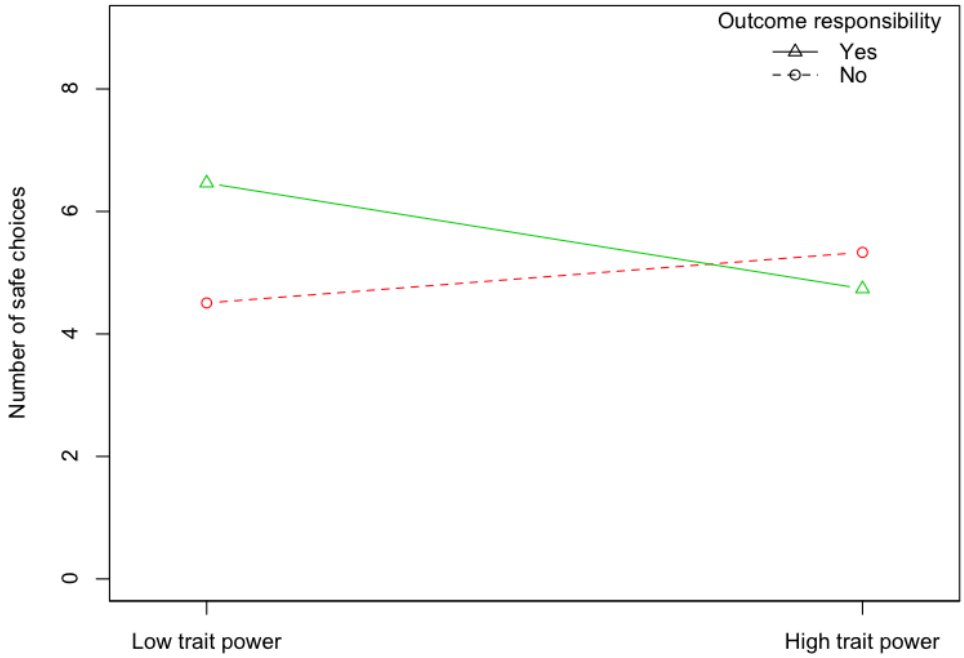

Figure 4.5. Effects of trait power and outcome responsibility on risk-taking

A significant effect was found for outcome responsibility, *b* = 8.23, *SE* = 3.12, *t*(73) = 2.64, *p* = .01, 95% CI [2.01, 14.44], meaning that when participants had responsibility they made more safe choices than when they had no outcome responsibility. In addition, a significant interaction effect was found, *b* = -7.70, *SE* = 3.14, *t*(73) = -2.45, *p* = .02, 95%CI [-13.96, -1.44], indicating that trait power and outcome responsibility interactively affected risk-taking (see Figure 4.5). Simple slope analyses showed that in the no-responsibility-condition, trait power did not influence the number of safe choices participants

made, b = 2.49, SE = 2.07, $t(35) = 1.20$, p = .24, 95% CI [-1.72, 6.70]. In contrast, in the responsibility-condition, we found a negative relationship between trait power and the number of safe choices, b = -.5.21, SE = 2.36, $t(38) = -2.21$, p = .03, 95% CI [-9.98, -0.43]. This pattern of results does again not corroborate our hypothesis. It rather suggests that our outcome responsibility manipulation was not able to increase participant's sense of responsibility beyond the amount that is per definition tied to power, whereas the decisions of low power individuals were shaped by their experience of being responsible for other's outcomes or not.

Second, we regressed the mean situational power (standardized), the experimentally assigned outcome responsibility (0 = no responsibility, 1 = responsibility) and their interaction on the mean number of safe choices in the two power roles. No significant effects were found.

Exploratory Analyses: Interpersonal Accuracy

We tested the effects of the experimental manipulations on the two interpersonal accuracy measures with the mixed ANOVA described above. Table 4.3 in the Appendix shows main and interaction effects for both approaches, the absolute difference scores and the profile correlation scores. We had expected to find significant main effects of power, however we failed to do so with one exception: On the recall accuracy measure, participants in low power positions outperformed participants in high power positions. Unexpectedly, in all five analyses (i.e., difference scores and profile correlations with regard to thoughts and feelings plus profile correlation for the recall of nonverbal behaviors) the interaction of *Power* and *Order* was significant at least at $p < .10$ indicating on a descriptive level a general trend for participants to be more accurate (or at least never less accurate) in judging thoughts and feelings of their second interaction partner.

The test of whether the experimental groups' performance exceeds chance was a single-sample-t-test of the mean profile correlation against zero. Participants in the low power role were better than chance with regard to inferential accuracy and recall accuracy (thoughts: $t(62) = 4.47$, $p < .001$; feelings: $t(73) = 17.91$, $p < .001$; behavior: $t(76) = 3.50$, $p < .001$), whereas participants in the high power role were better than chance only for the inferential accuracy measures (thoughts: $t(65) = 1.88$, $p = .03$; feelings: $t(73) = 16.84$, $p < .001$; behavior: $t(74) = 0.86$, p = .20). Taken together, these findings indicate that individuals with low power tend to be at least a bit more accurate in rating their counterparts.

Discussion

Previous power research has almost exclusively relied on between-person designs in which participants assumed either a high power or a low power role. However, this is at odds

with the everyday reality of people experiencing context changes and it leaves theoretical questions about the dynamics of power unanswered.

The goal of this paper was to expand the foundations for studying power within person. We proposed to split psychological power (i.e., the subjective assessment of one's own ability to influence others in a given social situation) into its stable (trait power) and dynamic components (situational power resulting from structural power, i.e., objectively demonstrable control of resources valued by others, and other situational influences). We discussed the role of time, direction of change (i.e., power gain vs. loss) and individual differences in the interplay between trait power, structural power and psychological power. Based on these theoretical considerations we presented initial empirical evidence showing that gain in power and loss of power differ in their impact on affected (student) individuals.

In our study, participants assumed first either a structurally high power (power-loss-condition) or low power position (power-gain-condition) in a newly developed role-play task and changed in the second round their roles and interaction partners. Subsequently to each role-play participants completed measures of risk-taking and interpersonal accuracy. Within the risk-taking measure we manipulated outcome responsibility between participants, that is, whether the risky decision affected only the decision maker or both the decision maker and the interaction partner in the role-play.

Unexpected Results of Preregistered Analyses

Contrary to our preregistered expectations, the order in which participants played the two power roles mattered. In the power-loss-condition, participants did not differ in their self-rated power in the high and low power roles. However, in the power-gain-condition, participants reported feeling significantly more powerful in the high power role than in the low power role. In a similar vein, risk-taking behavior differed in the two order conditions. In the power-gain-condition, participants who had outcome responsibility made more safe choices than participants without outcome responsibility. In the power-loss-condition, participants in the low power position without responsibility made more safe choices than the others. Taken together, the hypothesis of reduced risk-taking for participants with outcome responsibility was not supported.

Our hypothesis was based on the precondition suggested by prior research that power increases risk-taking. If the powerful, who per definition control the outcomes of others, were not prone to increased risk-taking to begin with, it would not make much sense to try to reduce the powerful's risk-taking by making their responsibility vis-à-vis their subordinates salient, as we attempted to do in this study. We provide two possible explanations why we were not able to replicate previous findings.

First, a notable difference to prior studies is that these studies have relied on non-interactive power manipulations such as episodic and semantic priming, not enacted role

assignment, and power posing (Anderson & Galinsky, 2006; Carney et al., 2010; Fast et al., 2009; Fast et al., 2012; Hiemer & Abele, 2012; Jordan et al., 2011; Maner et al., 2007; Ronay & Von Hippel, 2010) in contrast to the current study that manipulated power via a role play. Different manipulations could trigger different mechanisms that could differentially affect outcomes (Tost, 2015). Our choice of manipulation could also explain why our power manipulation was only successful in the power-gain-condition. Maybe it is for participants in the case of other manipulations more obvious whether they have high or low power without needing to also experience the opposite.

Second, another noteworthy difference between our study and prior studies on power and risk-taking is that we increased the stakes and created more uncertainty. For instance, in the study by Carney et al. (2010), participants had a 50:50 chance of winning four instead of two dollars, in the first experiment by Fast and colleagues (2009) participants could win five dollar if they correctly predicted the outcome of a single roll of a six-sided dice. In our study, participants only knew that the maximum variable amount they could get was about the equivalent of 20 dollars and that this variable amount paid was the result of playing one randomly selected lottery among all their decisions on which lottery they wanted to participate in.

Difficulties in Manipulating Outcome Responsibility

In instructing participants that their decisions would determine their own and their interaction partner's variable compensation, we used an operationalization of outcome responsibility that is established within prior risk-taking research (Charness & Jackson, 2009; Bolton et al., 2015; Pahlke et al., 2015, Study 1; Wang et al., 2018). Thus, we would have expected that this manipulation affected the responsibility manipulation check. However, this was not the case, and instead the power manipulation impacted the responsibility manipulation check. This might be due to one of the following reasons: One the one hand, participants in our experiment might not have noticed this rather subtle manipulation (slightly different wording of the instruction). On the other hand, the outcome responsibility manipulation might have added no incremental sense of responsibility. The outcome responsibility manipulation took place subsequently to the power manipulation. The concept of power is already closely related to the concept of responsibility, as the powerless depend on the powerholders for important resources and powerholders are in the unique position to take care for others (Tost, 2015). In our study, the power-as-responsibility-interpretation (Sassenberg, Ellemers, Scheepers, & Scholl, 2014) might have been especially salient because we manipulated power by means of a role-play task and real participants were in the presence of each other for the whole experiment. This idea is also compatible with (1) the results of our internal analysis showing that the responsibility manipulation only made a

difference for participants low in trait power and (2) the finding that the outcome manipulation check varied as a function of the power manipulation and not the responsibility manipulation.

Future research should try to disentangle power and responsibility more thoroughly. For instance, instead of using one power manipulation and embedding the responsibility manipulation in the outcome measure as we did in this study, a better approach might be to use two power manipulations that differ in their ability to make responsibility salient (e.g., not enacted role assignment and role-play with real interaction partners) and use the identical outcome measure in both power conditions.

On a more abstract level, being aware of one's responsibility does not mean that one also acts upon it. Results of a study in which both self-/other-focus and power were manipulated suggest that even though power holders became more aware of their responsibility than those low in power after adopting an other-focus, this did not seem to change the outcomes, namely the perceived relationship to their counterpart in terms of objectification and interpersonal closeness (Scholl, Sassenberg, Scheepers, Ellemers, & de Wit, 2017, Study 2).

Exploration of Power's Effects on Interpersonal Accuracy

In our study we examined inferential accuracy with regard to thoughts and feelings of a real interaction partner as well as recall accuracy with regard to recalling the interaction partner's (non-)verbal behavior. We did not find systematic differences in inferential accuracy between participants as a function of their hierarchical position. However, albeit failing to reach conventional levels of significance, on the descriptive level there was a tendency for participants to be more accurate in assessing their second interaction partner's thoughts and feelings. Practice effects are a known disadvantage of within-person designs.

Interestingly, we did not find practice effects for recall accuracy, but effects that are in line with the theoretical prediction of increased interpersonal accuracy in the powerless (Keltner et al., 2003; Magee & Smith, 2013). Given that recall accuracy is not subject to the confounding effect of differences in expressive clarity (Hall et al., 2015) and also given that the study setting (with participants in powerless positions focusing on the puzzle and participants in powerful positions free to focus their attentional resources on their interaction partner) increases the likelihood of the opposite effect, we see our finding as evidence for the claim by the approach/inhibition theory and the social distance theory.

Suggestions for Future Within-Person Power Research

In the empirical part of this article we focused on the question whether power gain and power loss would produce different effects. We chose this question among the questions presented in the theoretical background as it can be addressed with relatively simple research designs. Participants in our study played the same tangram-building task twice,

once as decision maker directing another participant (i.e., high power role), and once as assistant receiving instructions from the decision maker (i.e., low power role), whereby the order of roles was counterbalanced across participants. This newly created power manipulation contains critical determinants of prior role-play manipulations such as the powerful's mandate to direct the powerless (cf. legitimate power, French & Raven, 1959) and the powerful's possibility to evaluate the powerless (cf. reward and coercive power, French & Raven, 1959). In addition, we equipped the powerful with superior knowledge (cf. expert power, French & Raven, 1959) and designed our procedure in a way that participants in low power positions played their roles not in a familiar environment as they were the ones who had to change rooms (cf. stage setting, Raven, 1992). Nevertheless, our study setting was not a strong situation (Ashforth, Kreiner, & Fugate, 2000; for an example in this context see Chapter 3) in the sense that the situation allowed participants to behave in line with their personality dominance or individual beliefs on leadership. As we studied dyads of real participants and did not rely on confederates this could have distorted our manipulation. However, previous research has shown that role-play power manipulations work even when using rather weak situations (e.g., Schmid Mast et al., 2009, Study 1).

In this regard, the approach to manipulate power by means of economic games such as ultimatum or dictator game chosen by Sivanathan and colleagues (2008) is advantageous because personality differences have less influence in this non-interactive setting as compared to a conversation situation like in our study. Moreover, economic games allow for operationalizing power as continuum – in contrast to the prevailing dichotomous treatment of the power construct (Handgraaf, Van Dijk, Vermunt, Wilke & De Dreu, 2008; Rus, van Knippenberg, & Wisse, 2010). However, in economic games the manipulation and the outcome are by nature intrinsically tied to one another. Accordingly, this approach is not very flexible if one is interested in outcomes other than prosociality vs. selfishness.

The most instructive (but also most costly and quite complex) laboratory power manipulation would be one that follows the principles of a round robin design (Warner, Kenny, & Stoto, 1979). In a round robin design, all possible pairs of participants from a given set of participants interact. For instance, one could schedule five participants, A, B, C, D, and E on the same experimental session and then there would be four rounds, in which participant A would interact with participants B, C, D, and E (participant B with A, C, D, and E and so on) and could play the high power role twice and the low power role twice. For each participant paired with every other participant, an observation could be made of the outcome variable of interest. This design would allow for (1) the observation of individual differences among participants (such as e.g., trait power), (2) a more reliable measurement of effects of power gain and loss as – depending on the number of interactions – more than one gain and loss episode can be investigated and (3) relatedly due to multiple interactions in the same

power position effects of specific interaction partners are controlled for (e.g., participant A was in the low power position in the interactions with B, C, and D; C and D behaved in line with their high power role, but participant B was very shy and overstrained by his high power role therefore A was forced to leave his assigned low power position).

However, questions of within-person power dynamics could also be investigated in the field using intensive longitudinal methods (also known as e.g., everyday experience methods, experience sampling, momentary ecological assessment). A key characteristic of these methods is to sample a person's thoughts, feelings and behaviors repeatedly across a relatively brief period of time (Fisher & To, 2012). This approach could be especially well suited to answer questions on the role of time in power dynamics (see first meta question in theoretical background). For instance, let's assume that one wants to find out how long it takes for individuals to disengage from a specific level of power and drop back to their habitual level of psychological power. One could design an experience sampling study that makes use of two sampling strategies, event-contingent and signal-contingent recording (Reis, Gable, & Maniaci, 2014). Event-contingent recording means that a participant has to complete a questionnaire whenever (s)he experiences an event that matches predefined criteria; in the current example this could be an interaction with a superior (i.e., the actor is in a position of low structural power) or a subordinate (i.e., the actor is in a position of high structural power) at work. Signal-contingent recording entails that participants describe their activity at the moment when the signal is delivered; in the current example one could send participants signals in predefined (or adaptive?) distances to the reported event and ask them extremely briefly how they feel in this moment on the three most basic emotional dimensions pleasure, arousal and dominance (e.g., by using the self-assessment manikin, SAM, Bradley & Lang, 1994). In this way one could keep track of psychological power's trajectory in response to an instance of high or low structural power.

To conclude, studying power within-person is an exciting new direction for future power research as there are still many theoretical questions open and established practices for manipulating power within-person are lacking. We hope to have made a valuable contribution to build a basis for further exploration of this issue.

CHAPTER 5: Contextual Variations in Social Power

Abstract

Experimental research conducted with student participants has documented that *feeling powerful or powerless* affects outcomes with high practical relevance for organizations. However, it is unclear how results from these studies can be generalized to organizational settings in which individuals have various roles that imply more or less *objective power*. To address this gap we present a theoretical framework to aid in the understanding of how objective power in organizations (i.e., structural power, situational power) affects psychological power (i.e., sense of power, personal power, sense of responsibility) with its various downstream consequences on outcomes. In contrast to previous research, we investigate power from a within-person perspective: Apart from being tied to stable hierarchical position in the organizational structure (between-person perspective), power also fluctuates dynamically within-person in reaction to an individual's interactions with people in different hierarchical positions. Results of a preregistered experiment (n = 190 participants) and a preregistered experience sampling study (n = 129 participants) conducted with non-student samples support the key predictions of our theoretical framework: Situational power is positively related to sense of power, which in turn is positively related to personal power and sense of responsibility. Effects of this objective-subjective power link are tested on the example of self-control. The dimensional investigation of self-control revealed that start, stop, and interpersonal self-control are affected differently by changes in power.

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Theoretical Background and Hypotheses

Power is a key aspect of social relationships. Psychological research has documented that having or lacking power impacts such diverse outcomes as self-control, goal pursuit, and interpersonal sensitivity (for reviews see Galinsky, Chou, Halevy, & Van Kleef, 2012; Galinsky, Rucker, & Magee, 2015; Guinote, 2017). Although the outcomes studied by power researchers are highly practically relevant for organizations, there is a lack of theory linking the experimental work to organizational reality (Schaerer, Lee, Galinsky, & Thau, 2018).

The preferred approach in social psychology has been to compare student participants in high-power, low-power, and sometimes control conditions in one-shot experimental designs. In doing so, power manipulations have mostly targeted *psychological power*, a psychological state characterized by the activation of a power-specific set of cognitive and behavioral tendencies (Galinsky, Gruenfeld, & Magee, 2003). It is unclear how results from these studies can be generalized to organizational settings in which individuals have various roles that imply more or less *structural power*, i.e., the objectively demonstrable control of valued resources tied to a stable hierarchical position. The main aim of the present paper is to fill this gap. We present a theoretical framework to aid in the understanding of how structural power in organizations impacts psychological power with its various downstream consequences. The second aim of the paper is to provide initial empirical evidence for key assumptions of the theoretical framework.

In the attempt to transfer social psychology's knowledge on power to organizational settings, two main aspects must be considered. First, the contexts of taking part in an (online) experiment or wielding power in real (organizational) life differ considerably (Schaerer et al., 2018). For instance, organizational power usually implies being held accountable, making decisions that are meaningful for subordinates (e.g., wages, promotions, holidays), and having to face the affected people again, whereas experimental power in its default version has no real consequences. Second, in contrast to experimental settings in which an individual experiences only one kind of hierarchical position (i.e., high power *or* low power), people in the real world change their hierarchical position during the course of a working day and/or their professional career. Taking both these aspects into consideration, we investigated the effects of power fluctuating within person in reaction to the individual's interactions with people in different hierarchical positions within their natural context - a phenomenon we will refer to in the following as *contextual variations in power*.

In the first part, we develop propositions about contextual variations in structural power, psychological power, and sense of responsibility in organizational settings. A core characteristic of contextual variations in social power is that they occur within-person, which is at odds with experimental research practice. We therefore devote a section to discussing the particularities of studying power within-person. To illustrate the relevance of contextual

variations in power, we discuss their effects on self-control as an exemple. Self-control is an important component of organizational life, as employees are constantly faced with the need to work in the service of long-term goals and to overcome conflicting desires in order to complete their tasks successfully (Lian, Yam, Ferris, & Brown, 2017). All propositions are presented in the schematic overview in Figure 5.1. In the second part, we test some of our propositions empirically.

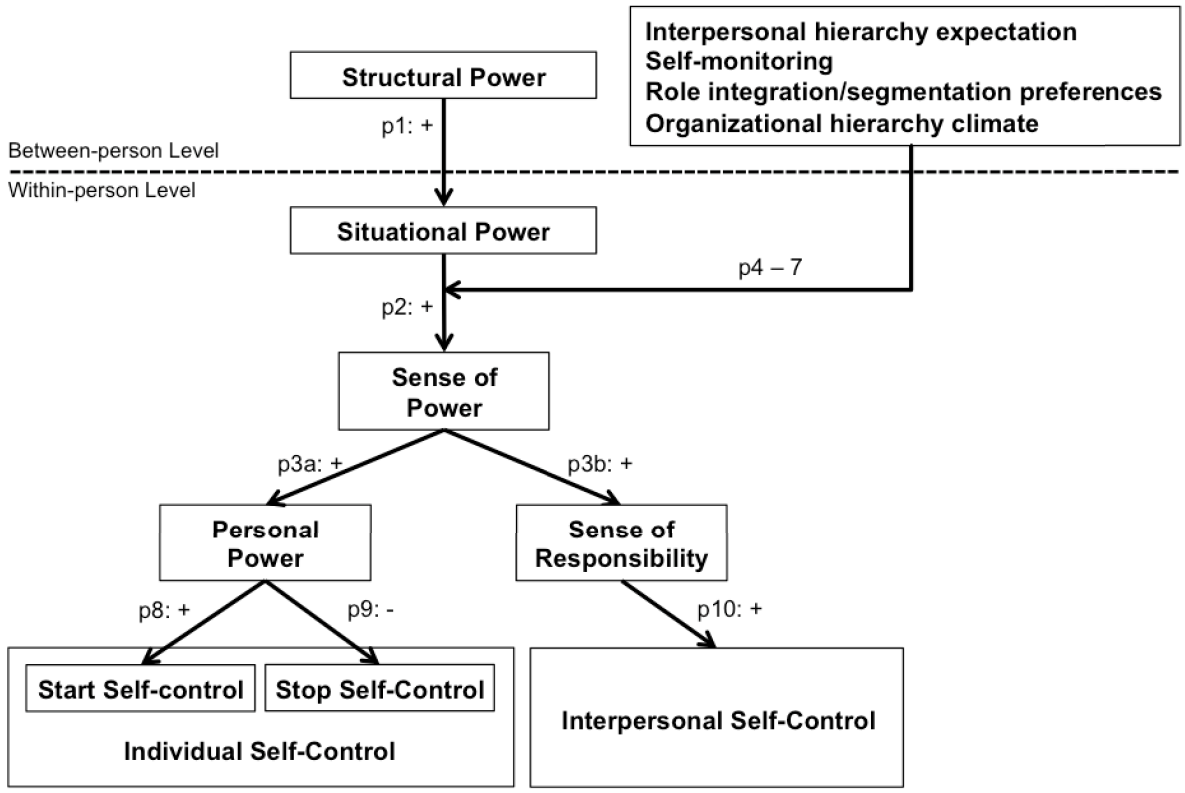


Figure 5.1. Theoretical model linking structural power to psychological power and self-control as exemplary outcome

Dimensions of Power

Power can be defined as the ability to influence others' behavior due to asymmetrical control over valued resources (Fiske & Dépret, 1996; Keltner, Gruenfeld, & Anderson, 2003; Lammers, Stoker, Rink, & Galinsky, 2016; Sturm & Antonakis, 2015; Thibaut & Kelley, 1959). However, this concept is too broad to be useful in bridging between experimental social psychology and organizational reality. Thus, we begin by clarifying our usage of various more specific concepts that can be subsumed under the broad concept of power (for an overview, see Table 5.1).

We use two related concepts to refer to the objective state of having more or less power. Power can manifest in *structural power*, the objectively demonstrable control of valued resources (Tost, 2015). This includes managerial authority or being in charge of

decisions about wages, bonuses, and promotions. Structural power translates into a stable hierarchical position in the organization chart. While structural power is (relatively) stable, *situational power* is potentially highly variable. When two or more individuals interact with each other, their situational power is determined by their relative structural power. Thus, when individuals' interaction partners come from different levels in the organizational hierarchy, their situational power changes.

Table 5.1
Relevant concepts, definitions and examples

	Concept	Definition	Examples
Objective Power	Structural power	Objectively demonstrable control of valued resources (Tost, 2015), tied to a stable hierarchical position in the organizational structure	Managerial authority, being in charge of decisions about wages, bonuses, and promotions, span of control
	Situational Power	Superiority of one's own or the other's stable hierarchical position within a specific social interaction of two or more individuals.	A works in a team with B and C that is supervised by D. When A interacts with D, A is in the subordinate role and D in the superior role. When A interacts with B, both are in a peer role, i.e., on the same hierarchical level.
Subjective Power	Sense of power	Conscious subjective judgment about one's own ability to influence others in a given social situation (Anderson, John, & Keltner, 2012)	• I can reward and/or punish the other to a greater extent than the other way around. • "I can get him/her/them to do what I want" (Anderson et al., 2012)
	Personal power	Conscious subjective judgment about one's own competence, agency, autonomy, and independence (Overbeck & Park, 2001; Van Dijke & Poppe, 2006; Lammers, Stoker, & Stapel, 2009)	• I can impact on things in my environment. • I can do things or make decisions without another person being able to interfere. • „I had the feeling that I was independent from other people" (Lammers et al., 2016)
	Sense of responsibility	Concern for others' well being and awareness of the consequences of own actions for others (Tost, 2015)	• "I am always thinking about how my actions will affect others" (Schaumberg & Flynn 2012) • "When making these decisions I need to take care of others' needs" (Scholl, Sassenberg, Scheepers, Ellemers, & Wit, 2017)

We use three related concepts to refer to the subjective experience of power. *Sense of power* describes a conscious judgment about one's own ability to influence others in a given social situation (Anderson, John, & Keltner, 2012), which results from situational power. *Personal power* is a conscious judgment about one's own competence, agency,

autonomy, and independence (cf. Overbeck & Park, 2001; Van Dijke & Poppe, 2006; Lammers et al., 2009). Although personal power may result from feeling a sense of power ("power over") at a given point in time, it also reflects a more generalized perception of "power to" that can also arise from non-social control over outcomes (Overbeck & Park, 2001). *Sense of responsibility* describes a state of being concerned for other's well being and an awareness of the consequences of one's own actions for others (Tost, 2015).

The relationship between structural power and situational power is key to our theorizing about contextual variations in power. Although relatively high levels of sense of power have been linked to high managerial positions (supplementary analyses of data by Lammers, Stoker, & Stapel, 2010), it is important to point out that organizational behavior is situated in diverse interaction contexts. For instance, individuals in a high structural power position, such as a CEO or members of the advisory board, interact mostly with people who have less influence and resources. Accordingly, these individuals should often find themselves in a superior role and have a relatively constant experience of a high sense of power. Individuals in a low power position, e.g., clerks, mostly interact with people who have equal or more influence and resources than they do. Hence, these individuals should often find themselves in a subordinate role and are more likely to experience a rather low sense of power. People situated in the middle level of an organizational hierarchy frequently switch between superior and subordinate interaction partners. Therefore, they will probably change between states of higher and lower sense of power from situation to situation, which could aggregate to a sense of middle power when they are asked to make a global self-assessment (Anicich, & Hirsh, 2017).

We assume the existence of two pathways linking power to outcomes (cf. Tost, 2015): The sense of power can unfold its effects via the perception of *personal power* and via the *sense of responsibility*. Per definition, individuals experiencing a high sense of power get others to comply with their own wishes. Thus, they experience that they have an impact and should accordingly feel agentic, competent, and autonomous (i.e., high personal power). At the same time, they might recognize that others' outcomes depend on their actions, especially because their superior position encompasses unique actual (or felt) means and possibilities to do good or avoid harm in the interest of others (Scholl et al., 2017). This perception of other's dependency should result in a high sense of responsibility. However, we agree with Tost (2015) that individual differences might moderate the relationship between sense of power and sense of responsibility such that women and individuals high in other-orientation might feel more responsible when in positions of power than men and more self-oriented individuals.

Proposition 1: Structural hierarchical positions (i.e., structural power) determine the probability to have a certain situation-specific power role in interactions (superior, peer, subordinate; i.e., situational power).

Proposition 2: Situational power translates to a sense of power: When interacting with a superior, an individual will have a lower sense of power than when interacting with peers or subordinates. When interacting with a subordinate, an individual will have a higher sense of power than when interacting with peers or superiors.

Proposition 3: The sense of power is positively related to (a) personal power and (b) a sense of responsibility.

From Propositions 1 – 3, it is tempting to derive the conclusion that organizational members of relatively high ranks in the organizational hierarchy should chronically experience higher personal power and sense of responsibility than organizational members of lower ranks. As noted by Tost (2015) however, all three subjective (or psychological) forms of power are causally determined in multiple ways, including non-conscious influences, weakening the link from structural power to personal power and sense of responsibility. If the goal is to predict subjective power, a dynamic (or within-person) perspective beginning the causal chain at situational power would seem to be more appropriate.

Within-Person Power Changes

In experimental research, little is known about the dynamic nature of power (for scarce exceptions see Goodwin, Gubin, Fiske, & Yzerbyt, 2000, Study 3; Li, Wang, Yin, Mao, Zhu, & Huang, 2016; Sivanathan, Pillutla, & Murnighan, 2008; Weick, McCall, & Blascovich, 2017, Study 2). However, these few studies do suggest that participants alter their behavior as a response to changes in their structural and psychological power in laboratory settings. For instance, Goodwin and colleagues assigned each participant to three different roles – a powerful allocator role, a powerless receiver role, and a power-neutral observer role – and asked them to form an impression of people with whom they would ostensibly work together in an upcoming task based on descriptive statements about these people. The amount of time participants spent reading and processing the different statements constituted the dependent measure. Although the amount of attention toward the target did not vary as a function of participants' role, they focused on stereotype-inconsistent information when in the powerless role but changed their focus to stereotype-consistent information when in the powerful role.

Initial evidence suggests that within-person changes in power also have effects in real-world settings. The only published study in this category – replicating previous findings established in between-person designs in the laboratory – showed that having power is related to positive mood, less resource depletion and less stress (Smith & Hofmann, 2016). However, results also revealed that positional power (corresponding to situational power in

our terminology) imperfectly predicted feelings of power (which corresponds to subjective power in our terminology). This emphasizes the importance of subjecting this link to further scrutiny. After giving a brief overview on previously discussed moderators involved in perceptions of positional security, we will discuss some individual characteristics as well as properties of the context that might determine to what extent situational power predicts subjective power. These newly proposed moderators share the idea that they are thought to amplify the perceptions of power differences between hierarchical positions, which should ultimately result in stronger effects on outcomes.

Perceptions of positional security

How secure individuals perceive their hierarchical position to be might affect how situational power translates into psychological power. Security in this context is the result of multiple sources: legitimacy, interdependence, and stability, which have previously been theorized (e.g., Keltner et al., 2003; Tost, 2015; Williams, 2014) and shown (e.g., Lammers, Galinsky, Gordijn, & Otten, 2008; Sligte, De Dreu, & Nijstad, 2011) to alter the effects of power. Importantly, in order for all these moderators to take effect, the self-perception of the individual in question is what matters. The objective circumstances can be neglected.

Legitimacy describes the perception that actions of an agent are in line with a shared understanding of what is desirable, appropriate and compliant to norms and values (Raven, 1992, Suchman, 1995). For instance, within organizational settings, leader selection or promotion decisions can give rise to perceptions of (il)legitimacy: power positions can be gained legitimately based on fair and recognized factors such as competence or performance. However, they can also be gained illegitimately based on luck, nepotism, or sexual favors. Not having earned one's power position for the right reasons or merely suspecting that others might think one does not deserve one's power position might evoke feelings of insecurity and diminished self-confidence in the powerholder, which could result in reduced subjective power. In contrast, being in a powerless position illegitimately might evoke a sense of moral superiority that could bolster positive self-perception and increase subjective power.

Interdependence refers to a situation in which both actors control resources that are mutually important for the other actor (Langer & Keltner, 2008; Thibaut & Kelley, 1959). For instance, when CEOs want to implement a new wage plan that would save their companies money but disadvantage the workers, the CEOs have the formal power to do so but will probably not evaluate their power to be high if a charismatic, experienced worker or unionist convincingly threatens to organize a strike. Meanwhile, the assistants to the CEO might not feel as powerless as one would suppose based on their inferior structural power position because they also control resources their superiors depend on. These include work-related

skills, knowledge of administrative processes, or contacts to important internal and external stakeholders.

Finally, stability might play a role for the evaluation of subjective power. Powerholders may feel secure in a stable power position. However, after learning that ambitious subordinates are trying to topple them to take over their position, or that a superior is likely to dispose of them in favor of a younger and less expensive candidate, they might feel less secure and less powerful. In contrast, the powerless might perceive the instability of the situation as a chance to strengthen their own position (e.g., showing how competent they are when the reason for a possible displacement of their superior is a lack of competence) or interpret it as a chance to rise in hierarchy themselves. Hence, someone with unstable low situational power could report higher subjective power than someone with unstable high situational power.

Interpersonal hierarchy expectation

People high in interpersonal hierarchy expectation (IHE) act on the assumption that dominance hierarchies are present or develop in interpersonal interactions or relationships (Schmid Mast, 2005). Based on the mechanism of self-fulfilling prophecy, hierarchy-expecting individuals might behave in line with the assumed power balance between them and another person, which in turn causes the interaction partner to behave complementarily (Tiedens & Fragale, 2003; Tiedens, Unzueta, & Young, 2007). The interaction partner's complementary behavior reinforces the (assumed) hierarchy.

Consider for instance employee A and employee B, who both have separate meetings with their supervisor. While all other parameters are kept constant (tenure, gender and so on), employee A, who values hierarchy and acts in line with this guiding principle, is likely to perceive the power difference between him/her and his/her supervisor as larger than employee B, who does not believe in the value of hierarchy and accordingly meets his supervisor without deference. When both A and B subsequently speak to the trainee they are responsible for, A might feel the need to appear as a demanding and competent superior, whereas B tends to see the relationship between superior and trainee as cooperative relationship between peers. To summarize this example, the distance A covers to change from the subordinate role to the superior role is larger than the distance B covers for the same change. The "power manipulation" in the case of A is stronger and the effects of power on outcome variables should therefore also be stronger for A.

Proposition 4: The effects of contextual variations will be more pronounced among employees high in interpersonal hierarchy expectation (IHE) than employees low in IHE.

Self-monitoring

Self-monitoring refers to the extent to which people observe, regulate, and control the public appearances that they display in social settings and interpersonal relationships (Gangestad & Snyder, 2000; Snyder, 1987). High self-monitors are particularly sensitive and responsive to (social) cues in their environment indicative of situationally appropriate behavior. In contrast, low self-monitors tend to be true to themselves and display more consistent behavior across various social contexts.

In an organizational context various cues might tell a high self-monitor where his place in the hierarchy is and what sort of behavior is expected. For instance, the most proximal cue is the interaction partners' behavior. If they appear higher ranking – be it due to their dress (Kraus & Mendes, 2014), their way of speaking (Hall, Coats, & Smith LeBeau, 2005; Schmid Mast, 2002) or their nonverbal behavior (Hall et al., 2005) – it would be appropriate to behave as a subordinate. A more distal cue is the organizational hierarchy climate (see section below). If it emphasizes the importance of hierarchy, high self-monitors are likely to strongly adapt their behavior when interacting with subordinates or superiors.

Proposition 5: The effects of contextual variations in power will be stronger among high self-monitors than low self-monitors.

Role integration/segmentation preferences

Role integration and segmentation constitute a continuum that describes the degree to which aspects of one domain are kept separate from the other domain (Ashforth, Kreiner, & Fugate, 2000). High segmentation is characterized by inflexible and impermeable role boundaries whereas high integration is characterized by flexible and permeable role boundaries. For instance, a supervisor who prefers segmentation might avoid having blended meetings with both her subordinates and her boss as well as she might have clear rules about her communication style and content with her subordinates and her boss (e.g., never be on first-name terms with subordinates, never tell subordinates about her family). In contrast, a supervisor who prefers integration might address both his subordinates and his boss informally, share personal stories with his subordinates as well as his boss and prefer to attend meetings with all concerned parties regardless of their hierarchical position. Individuals differ in their preference for highly integrated or highly segmented roles (Edwards & Rothbard, 2000; Rothbard, Phillips, & Dumas, 2005).

The way members of an organization have organized their roles at work determines the magnitude of change they experience when moving from one role to the next. Role integrators will perceive little change and the "power manipulation" for them will be less strong than for role segmentors, who might appear as different people as a function of their interaction partner.

Proposition 6: The effects of contextual variations in social power will be stronger among employees who adopt a role segmentation strategy than among employees who adopt a role integration strategy.

Organizational hierarchy climate

Organizational climate, the "summary perception derived from a body of interconnected experiences with organizational policies, practices and procedures and observations of what is rewarded, supported, and expected in the organization" (Schneider, González-Romá, Ostroff, & West, 2017, p. 468), has a fundamental influence on organizational behavior such as job attitudes, job performance, strain, turnover intentions, and organizational citizenship behaviors (Chang, Rosen, & Levy, 2009; Wallace, Edwards, Paul, Burke, Christian, & Eissa, 2016). Organizational climates might not only pertain to service, safety, justice, discrimination, and harassment (Schneider et al., 2017), but might also differ regarding the importance of hierarchy. In some organizations (or even industries: e.g., IT, Research vs. Consulting, Banking) differences in power might be very salient and actively cultivated.

For instance, people on different hierarchical levels might dress differently, there might be rules about the maintenance of professional distance between people on different hierarchical levels, communication might be regulated strictly in a way that does not allow skipping hierarchical levels in upward communication, or the rise in organizational hierarchy might be visibly accompanied by presents or privileges such as a (bigger) company car, a nicer office, or one's own secretary. In contrast, in other working environments, interns and vice presidents might dress similarly and get together for organizational and casual social functions easily and frequently regardless of job title. When working in an environment in which power differences are salient, an individual should easily know the difference between interacting with his subordinate or her boss and this could make him/her feel and behave differently.

Proposition 7: For employees working in organizations high in hierarchy salience the effects of contextual variations in social power will be stronger than for employees working in organizations low in hierarchy salience.

Power Effects in Organizational Contexts: The Example of Self-Control

Employees face many challenges throughout the course of a day. They must focus on their tasks and avoid being distracted by every incoming e-mail. They must tackle tasks they dislike. They must not allow themselves to be carried away to do or say spontaneously something inappropriate. They need to cultivate their relationships with their coworkers by taking on some of their workload or explaining them for the umpteenth time how to proceed in case X. These are demands that cannot be dealt with by routine and instead require the effortful and conscious control of thoughts, feelings, and behavior in order to comply with

performance goals, organizational values and norms as well as coworker, supervisor, and subordinate expectations (cf. Baumeister, Vohs, & Tice, 2007). In short, self-control is an indispensable part of working life.

As the above examples suggest, these self-control challenges can be clustered. Organizational psychologists proposed to discern impulse control from overcoming inner resistances and from resisting distractions (Schmidt & Neubach, 2010). Comparable two- or three-factorial conceptualizations based on functional processes, i.e., initiation, (maintenance), and inhibition, were also proposed by psychologists from other less applied disciplines (e.g., de Boer, van Hooft, & Bakker, 2011; de Ridder, de Boer, Lugtig, Bakker, & van Hooft, 2011; Hoyle & Davisson, 2016; review in Chapter 3). We borrow the idea of self-control as a multifaceted construct for our rationale: In the following, self-control that is needed for initiating goal-directed behavior is referred to as *start self-control*, whereas self-control needed for suppressing behavior or refraining from acting impulsively is referred to as *stop self-control*. What is obvious but rarely discussed from the above examples is that self-control is also related to interpersonal functioning and interpersonal relations, not only directed at behavior mainly concerning the self (i.e., *interpersonal self-control*; Olsen, 2005; Vohs, Lasaleta, & Fennis, 2009).

In the past decade, research on in-role and extra-role behavior at the workplace linked to the exertion of self-control (such as unethical behavior, deviance, workplace aggression, organizational citizenship behavior, job engagement, distraction; Lian et al., 2017) has revealed substantial within-person variability over days and weeks (e.g., Dalal, Lam, Weiss, Welch, & Hulin, 2009; Johnson, Lanaj, & Barnes, 2014). As predictors of this variability, situational characteristics such as affect, experiences of incivility, justice behaviors, stress, and sleep deprivation have been investigated (e.g., Barnes, Lucianetti, Bhave, & Christian, 2015; Dalat et al., 2009; Johnson et al., 2014; Meier & Gross, 2015; Spence, Ferris, Brown, & Heller, 2011). These effects have often been interpreted with reference to a resource-based account of self-control. However, more recent theorizing stresses the importance of motivation and attentional focus in explaining changes in state self-control (Inzlicht & Schmeichel, 2012; Kotabe & Hofmann, 2015), an idea based on which we will further develop our reasoning.

We propose that contextual variations in power could be an additional predictor of state self-control variability. For start and stop self-control, we draw on the motivational account advanced in the approach/inhibition theory of power (AIT, Keltner et al., 2003) and related theoretical developments in suggesting that people in different hierarchical positions concentrate their self-control efforts on the kind of self-control that best fits their motivational focus. This suggests that social power benefits start self-control but impairs stop self-control.

In brief, the approach/inhibition theory (Keltner et al., 2003) assumes that having power

activates the behavioral activation system (BAS), which is related to experiencing positive affect and being attentive to rewards. In contrast, lacking power activates the behavioral inhibition system (BIS), which is related to experiencing negative effect and focusing attention to threats. As people in states of low power are attentive to threat, punishment and others' interests, they should be focused on not doing anything wrong. The most straightforward way not to become a target of threat or punishment is to restrain oneself, i.e., not to violate norms, to down-regulate feelings and to think things through – which are core characteristics of what we term stop self-control. In contrast, people in states of high power are attentive to rewards. The attainment of self-relevant, self-set goals is rewarding. In order to successfully achieve one's goals, it is necessary to turn plans into action, concentrate on the goal and nothing else, and persist even in the face of obstacles – which are core characteristics of what we term start self-control. Importantly, we think that the subjective perception of power triggers these effects and not the objective perception of having or lacking structural power.

Proposition 8: The sense of power positively affects start self-control. This relationship is mediated by personal power.

Proposition 9: The sense of power negatively affects stop-self-control. This relationship is mediated by personal power.

In the interpersonal self-control domain, we expect that individuals in high power positions who recognize their responsibility towards their subordinates would be motivated to behave in a self-controlled way, e.g., not to yell at subordinates, not to say negative things to subordinates in the spur of the moment, not to be aggressive, and not to behave unethically at their expense. This is in line with past findings, which showed that powerful participants unaware of the responsibility component of their power do not care about others (Bendahan, Zehnder, Pralong, & Antonakis, 2015; de Cremer & van Dijk, 2005; Galinsky, Magee, Inesi, & Gruenfeld, 2006; Lammers, Stapel, & Galinsky, 2010). However, powerful participants who do perceive their responsibility act in ways that are beneficial for others (e.g., DeCelles, DeRue, Margolis, & Ceranic, 2012; Schmid Mast, Jonas, & Hall, 2009; Tost, Wade-Benzoni, & Johnson, 2015).

Proposition 10: The sense of power positively affects interpersonal self-control. This relationship is mediated by the sense of responsibility.

Present Research

So far we have presented various propositions related to the effects of contextual variations in power in an organizational setting. First, we have elaborated on the different facets of power: Power can be an objective property (i.e., structural and situational power) or a subjective perception (i.e., sense of power, personal power) of an interpersonal situation.

Situational power gives rise to a sense of power, which in turn affects various outcomes mediated by personal power and the sense of responsibility. Second, we have proposed that the magnitude of power effects caused by within-person power role changes common in organizational reality is dependent on individual and situational characteristics such as the organizational hierarchy climate, interpersonal hierarchy expectations, self-monitoring, and role integration/segmentation preferences. Third, we have discussed how real-world power would impact self-control, an example outcome relevant in organizational reality.

In the following, we present two studies that test key assumptions of our theoretical framework. As detailed above, we assume that the structural power differences between two interaction partners (i.e., situational power) determine how powerful the interaction partners perceive themselves to be (i.e., subjective power). Participants in Study 1 were asked to imagine themselves in the position of dealing with a subordinate, a peer or a superior and to answer questions on the thoughts, feelings and behavior they would be likely to have. In Study 2, an experience sampling study, participants reported their current state after meaningful interactions with other people in different hierarchical positions at their workplace.

Study 1

This study had three goals. First, we wanted to test in an efficient way whether information on one's own and the other's hierarchical role in a workplace interaction is sufficient to manipulate participants' self-assessment of the facets of power discussed above, i.e., whether this kind of information constitutes a successful "natural power manipulation" (see Proposition 2). We therefore simulated this situation in an online experiment, in which each participant had to report about situations in which (s)he adopted a subordinate, peer or superior role.

Second, we were interested in finding out how this manipulation affected self-control. We tested whether a high sense of power would increase start and reduce stop self-control and if this relationship was mediated by personal power (see Propositions 7 and 8)[10]. We focused on the comparison between high power (interactions with people from a lower hierarchical level) and low power situations (interactions with people from a higher hierarchical level) as we were unsure what to expect in the equal power situation. It is possible that people on the same hierarchical level still experience their interactions as hierarchical, therefore we did not know how participants would translate these experiences in one aggregate score.

[10] In the preregistration we stated the expectation that the sense of responsibility should be positively related to stop self-control. However, this was not the case. The reason might be that our stop self-control scale contained ambiguous items that could be understood as referring to either individual or interpersonal self-control. As interpersonal self-control in our definition is predominantly stop self-control (e.g., overriding selfish and self-interested impulses, controlling own emotions in order not to hurt or frighten others), we did not stress the difference between individual and interpersonal self-control in formulating the items. So, it might still be that the sense of responsibility would mediate the relationship between sense of power and *interpersonal* stop self-control, but not the relationship between sense of power and *individual* stop self-control. This issue will be addressed in Study 2.

Third, our goal was to investigate effects of within-person variations in power in organizational settings. Accordingly, in order to design Study 2 appropriately, we needed background information on the phenomenon's base rate (i.e., how often do employees experience power role changes?), sample characteristics (i.e., who is most likely to experience a fair amount of changes?), and effect sizes.

Method

The preregistration of this study can be accessed via https://osf.io/cgu47/. Data and materials can be accessed via https://osf.io/3b6wr/.

Sample

We recruited as many working adults as possible between November 10 and December 7, 2017 via personal contacts, various mailing lists and electronic bulletin boards, to take part in an online study on role changes in the workplace. We committed ourselves to terminating data collection before analyzing the data. In total, 316 individuals gave their consent to participate, of which 123 individuals had to be excluded because they did not provide answers to the key questions presented in the measures section. Additionally, three individuals were excluded because they gave identical answers to the key questions, which we interpreted as unserious responding.

The final sample consisted of 190 German speaking individuals (114 female, 76 male; 5 in top management, 20 in middle management, 22 in lower management, 60 experienced employees, 83 employees[11]) in the age range between 19 and 63 years (M_{age} = 32.85, *SD* = 12.08) who reported *Md* = 38.5 (Range: 2 - 60) working hours per week in their main jobs. As compensation, participants could win one of six vouchers for various online stores, worth 20 EUR/Swiss Francs each (approximately 23.50 USD).

Power manipulation

Participants were asked to imagine dealing with people at the same, higher, or lower hierarchical level and to indicate how they would feel and act in this situation in general. For instance, when a participant imagines an interaction with someone who belongs to a lower hierarchical level, the focal participant is in a high power position. Participants were instructed to give their ratings per item, i.e., answer one item with regard to all three hierarchical positions in succession and only then proceed to the next item.

Measures

The items for the five dependent constructs *sense of power*, *personal power*, *sense of responsibility*, *start self-control* and *stop self-control* were formulated based on the construct

[11] We clearly defined what we meant by the five hierarchical levels participants were asked to assign themselves to. We differentiated between top, middle and lower management, experienced employees who temporarily take on the lead or act as a role model, and employees without managerial responsibilities.

definitions (cf. Table 5.1) and pretested in a pilot study. All items were answered on a scale ranging from 1 (not at all) to 5 (completely). There was always a reference to “the people mentioned below” in the item stem. These people were either “people above me in organizational hierarchy”, “people on my level in organizational hierarchy”, and “people below me in organizational hierarchy”.

Sense of power. Participants indicated to which extent they agreed to the following three items: “If I imagine to interact with the people mentioned below, I would...” (1) “control the resources (e.g., time, money, information) the other person needs”, (2) “be able to punish or reward the other person”, (3) “get the other person to do what I want”. Cronbach’s alpha was .49.

Personal power. Participants indicated to which extent they agreed to the following four items: “If I imagine to interact with the people mentioned below, I would...” (1) “have the feeling that I can impact on things in my environment”, (2) “have the feeling that I can do my work in a self-determined way”, (3) “have the feeling that I can do things and make decisions without the possibility that someone else can change it”, (4) “be free from the influence of the other person”. Cronbach’s alpha was .58.

Sense of responsibility. Participants indicated to which extent they agreed to the following four items: “If I imagine to interact with the people mentioned below, I would...” (1) “feel responsible for the other person’s well-being”, (2) “be concerned about the other person’s needs”, (3) “want the other person to benefit from my actions” (4) “consider the other person’s well-being to be very important”. Cronbach’s alpha was .72.

Start self-control. Participants indicated to which extent they agreed to the following four items: “If I imagine to interact with the people mentioned below, I would tend in this situation to...” (1) “put plans into action”, (2) “follow through with difficult or taxing tasks”, (3) “start working on new challenging tasks”, (4) “be able to complete tasks that need to be done even if I don’t feel like it.” Cronbach’s alpha was .63.

Stop self-control. Participants indicated to which extent they agreed to the following four items (the first two items were recoded): “If I imagine to interact with the people mentioned below, I would tend in this situation...” (1) “to let myself go”, (2) “to give free rein to my feelings”, (3) “not to be carried away to do spontaneously something inappropriate”, (4) “to try to control how I react to impulses (e.g., spontaneous thoughts, feelings)”. Cronbach’s alpha was .60.

Other measures. Participants also provided information on how often they changed hierarchical roles the day before and the week before. When answering these questions, participants referred to the following roles: lower-ranking coworker (staff, intern…), higher-ranking coworker (team leader, boss), coworker on the same hierarchical level, client/customer, and retailer/supplier/service provider. We explained that participants should

consider interaction situations that fit the following three criteria: The interaction should (1) not be purely private, i.e., it should be at least partially about work, (2) take place in real-time (i.e., face-to-face, over the phone, by messenger), and (3) have a certain significance (i.e., greetings or brief small-talk are of no interest).

Procedure

Participants provided informed consent and demographic information. Then, they answered questions related to their employment situation. Subsequently, we were interested in descriptive information regarding hierarchical role changes (i.e., how many in a normal working day). Finally, participants answered the questions on the facets of power and self-control for the three different situations. Participants were thanked and informed about the compensation.

Table 5.2
Intercorrelations of study variables

	Low situational power	High situational power	Sense of power	Personal power	Sense of responsibility	Start SC
High situational power	-.50***					
Sense of power	-.59***	.62***				
Personal Power	-.62***	.50***	.63***			
Sense of responsibility	-.53***	.38***	.56***	.45***		
Start SC	.32***	-.23***	-.26***	-.25***	.00	
Stop SC	.54***	-.17***	-.36***	-.47***	-.36***	.29***

Note. Stop SC = Stop self-control, Start SC = Start self-control. N = 570 observations of 190 participants. All variables but situational power were person-mean centered to allow comparisons with Study 2. The two situational power variables were dummy-coded such that interactions with peers are the reference category.
*** $p < .001$. ** $p < .01$. * $p < .05$.

Results and Discussion

Table 5.2 shows the correlations between the dependent measures. The correlations between the measures of subjective power were positive and hence in line with our expectations. Also confirming our expectations, both sense of power and personal power were negatively correlated with stop self-control. Surprisingly, we also found sense of power and personal power to be negatively correlated with start self-control. With regard to the two

self-control measures we replicated previous findings showing a weak positive correlation (e.g., Hoyle & Davisson, 2016).

Table 5.3 shows the descriptives for the three experimental conditions. The sense of power scale can be considered a manipulation check. As expected, participants reported having more power in the high power role than in the low power role, $t(189) = 15.87$, $p < .001$, $d = 1.15$. Thus, manipulating power by referring to the changing roles people assume as a function of their interaction partners seems to be a promising approach. We made no prediction regarding the peer situation but found the sense of power to be at an intermediate level between the ratings for high power and low power positions.

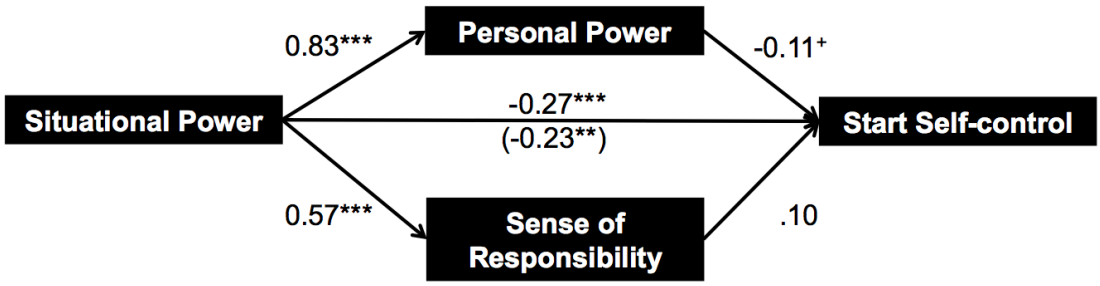
Table 5.3
Means and standard deviations (in parentheses)

Situational power	Low power role	Peer role	High power role
Sense of power	2.27_a (0.73)	2.76_b (0.71)	3.32_c (0.83)
Personal power	2.76_a (0.74)	3.31_b (0.61)	3.59_c (0.67)
Sense of responsibility	3.46_a (0.81)	3.89_b (0.70)	4.04_c (0.72)
Start self-control	3.98_a (0.68)	3.76_b (0.64)	3.69_b (0.70)
Stop self-control	4.09_a (0.66)	3.56_b (0.72)	3.67_c (0.73)

Note. In one line, conditions with different subscripts differ with at least $p < .05$.

Main analyses

To test the hypothesized main effects, we conducted paired t-tests of the scale measures of start and stop self-control. Contrary to our hypothesis, we found that participants reported more start self-control when low in situational power than when high in situational power, $t(189) = 5.60$, $p < .001$, $d = 0.41$. However, in line with our expectations, participants reported more stop self-control when low in situational power than when high in situational power, $t(189) = 9.25$, $p < .001$, $d = 0.67$. A post-hoc power analysis for this main effect's hypotheses suggested that our sample size of $n = 190$ allows for the detection of an effect of (at least) $d = 0.26$ with 95% statistical power, assuming two-sided tests and an alpha error of 5%.



*** $p < .001$. ** $p < .01$. * $p < .05$. + $p < .10$

Figure 5.2. Results of meditational analyses for start self-control

We used the analytical framework described in Judd, Kenny, and McClelland (2001) to test the within-person mediation hypotheses. We regressed the difference score of the dependent variable (either start or stop self-control) on the difference and (centered) sum scores of the two mediator variables (personal power and sense of responsibility). When the difference score is a significant predictor, this points to mediation. When the sum score is a significant predictor, this points to moderation.

With regard to start self-control, the results of the regression indicated that the four predictors explained 4.8% of the variance[12], $F(4,184) = 2.32$, $p = .06$. Both the intercept, $t(184) = 2.90$, $p = .004$, and the personal power difference score, $t(184) = 2.03$, $p = .04$, were significant, pointing to partial mediation (because of the still significant intercept). Figure 5.2 displays the results of the mediation analyses. The remaining three predictors (personal power sum score, responsibility sum, and difference scores) were not significant. Although we expected personal power to mediate the relationship between situational power and start-self-control, a negative effect does not conform to our expectations (cf. Proposition 8).

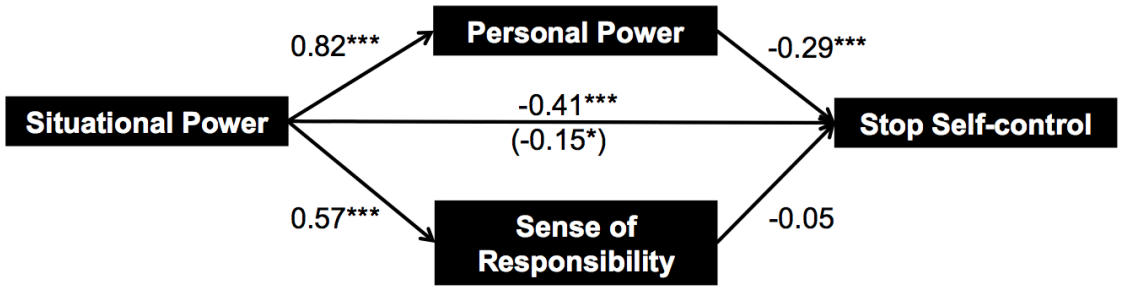


Figure 5.3. Results of meditational analyses for stop self-control

With regard to stop self-control, the results of the regression indicated that the four predictors explained 19.2% of the variance, $F(4,185) = 10.96$, $p < .001$. The intercept, $t(185) = 2.45$, $p = .02$, and the personal power difference score, $t(185) = 5.82$, $p < .001$, were significant predictors, indicating partial mediation. The personal power sum score was a marginally significant predictor, $t(185) = 1.86$, $p = .06$. Neither sum nor difference score of sense of responsibility were significant. Figure 5.3 displays the results of the mediation analyses. In line with Proposition 9, higher situational power was related to less stop self-control. As expected, personal power mediated this relationship.

[12] These were the results when one extreme outlier was excluded. In our preanalysis plan, we stated that we would report results separately in the case that the analyses with and without outliers yield different conclusions with regard to significance/non-significance of results (McClelland, 2000). Using the complete data set, the results of the regression indicated that the four predictors explained 7.1% of the variance, $F(4,185) = 3.53$, $p = .008$. The intercept, $t(185) = -.41$, $p < .001$, the personal power sum score, $t(185) = 2.24$, $p = .03$, and the responsibility difference score, $t(185) = 2.56$, $p = .01$, were significant predictors.

Analyses related to within-person variations in power: background information

Given their explorative nature, most results in this section are presented graphically. Most participants were familiar with the phenomenon in question: 139 out of 190 participants indicated to have experienced between 1 and 120 power role changes at work the day before (*M* = 7.32, *Md* = 4, *SD* = 11.6). In the preceding week, 155 participants experienced between 1 and 500 role changes at work (*M* = 26.5, *Md* = 15, *SD* = 47.79). It appears that participants on all hierarchical levels experienced role changes quite frequently (see Figure 5.4), with a (visually identified) tendency for participants in lower or middle management to report more role changes than non-management members. Additionally, Table 5.4 suggests that people who are older, work more hours per week, and have more coworkers in their department are more likely to experience hierarchical role changes.

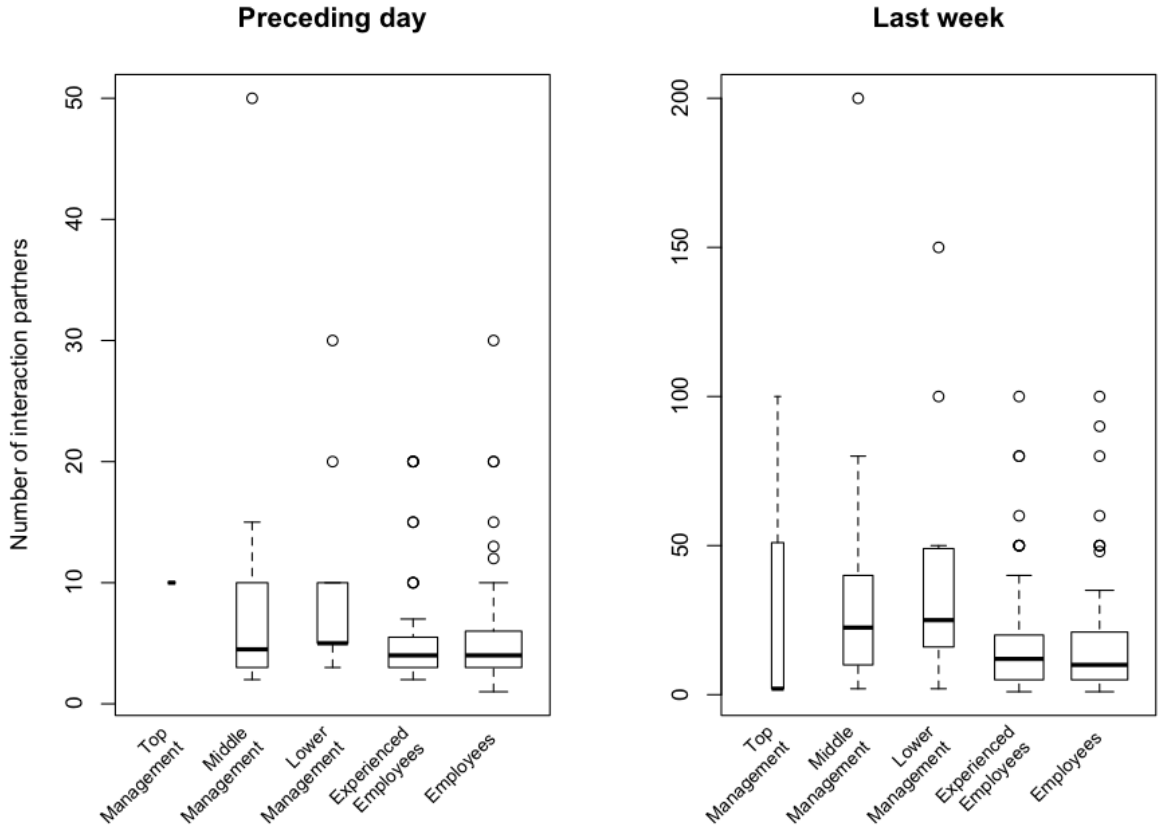


Figure 5.4. Number of role changes split by hierarchical position. The width of boxes is proportional to the sample size. For the sake of facility of inspection, we excluded one participant who gave the most extreme answers.

Given the extremely large range in reported role changes, we assume the median better reflects reality (even when working 60 hours per week, 500 hierarchical role changes conforming to our definition per week would mean that one has to have 8 interactions per hour on average). We cannot rule out the possibility that the large range is a result of imprecise definition – for instance, we did not specify how to count situations that include multiple interaction partners. Participants may also have had different interpretations of what to count. Consider Sarah's preceding day: She interacted with her colleague Mark, with her

colleague Mary, with her boss Tom, and the intern Claire. Given that our question was "How often have you changed between the above specified roles?", Sarah could have answered either "3" because she had the peer, superior, and subordinate role that day, or she could have answered "4" because she counts interaction partners rather than role labels.

Table 5.4
Correlations between the number of role changes and participants' background information

	No of changes yesterday	No of changes last week	No of direct reports	No. of coworkers in Department	Working hours	Female
No of changes last week	.96***					
No of direct reports	.08	.11				
No of coworkers in department	.13	.16^{+}	.10			
Working hours	.03	.14^{+}	.17^{+}	.12		
Female	-.06	-.12	-.01	.02	-.20**	
Age	.24**	.24**	.13	.06	.11	.08

*** $p < .001$. ** $p < .01$. $^{+}$ $p < .10$.

Study 2

In Study 1 we asked participants to *imagine* being in different hierarchical roles and to indicate how they *would tend* to feel or behave. This is very likely difficult because participants must remember multiple suitable situations, their inner states in these situations, to which they probably do not devote much attention on a normal day, and finally decide how to aggregate these experiences to give one single answer. Accordingly, participants' answers probably reflect beliefs rather than actual experiences.

To come closer to participants' actual experiences, we conducted an experience sampling study. Experience sampling (ES) is a method used to collect multiple observations of the same participants in their daily lives, usually several times a day for up to several weeks, with a focus on assessing variables that fluctuate over the short term such as affect, behavior, thoughts, and/or situational context (Fisher & To, 2012). Major advantages of this method are (1) the reduction of both recall bias and use of heuristics, and (2) the possibility to unconfound within- and between-person processes.

This study was designed to test our key assumption, namely whether changing real-world hierarchical roles (i.e., objective power) would have an impact on psychological power (Propositions 2 – 3b). In addition, we investigated whether variations in personal power

would explain variability in individual-focused start and stop self-control motivation (Propositions 8 and 9) as well as whether variations in sense of responsibility would explain variability in interpersonal self-control motivation (Proposition 10).

Method

The preregistration of this study can be accessed via https://osf.io/cbxhq. Data and materials can be accessed via https://osf.io/3b6wr/.

Sample

We recruited 129 participants (69 females, 60 males, M_{age} = 39.96, SD_{age} = 12.01, range: 23 – 63 years) working for different organizations in different domains. Among them, 6 had a position in top management, 22 in middle management, 20 in lower management, 46 were experienced employees, 21 were employees, and 14 did not find a suitable category to describe their position. The majority had at least a university or polytechnic degree (70%) and worked full time (68%). Our inclusion criteria (based among other considerations on the insights from study 1) were the following: Participants had to (1) be German-speaking, (2) possess and use a smartphone with Internet access, (3) be employed (not self-employed, retired or seeking employment), (4) have a certain amount of latitude with regard to the temporal organization of the own work, (5) work maximum one day per week from home, (6) work for an organization with at least 5 employees at their site, and (7) to have no psychological diseases such as e.g., depression, burnout.

Our data initially encompassed 1649 observations. We excluded (1) observations that were reported between 10 p.m. and 5 a.m. (2.8%) because given that all participants were employed in Europe, it is highly unlikely that they actually experienced work-related interactions within these times and (2) observations that included only an answer to the very first question of the questionnaire (1.6%). Our final sample comprised 1577 observations from 129 participants. Among all observations, 18% referred to high power situations, 30% to equal power situations, 25% to low power situations, 16% to interactions with customers, 4% to interactions with suppliers, and 7% to other kinds of interactions. Our analyses are based solely on observations referring to interactions with subordinates (= high power situations), peers (= equal power situations) and own superiors/other superior colleagues (= low power situations), of which participants provided 8.83 observations on average (*Md* = 8, *SD* = 4.79, range 1 – 21). Viewed differently, 15% of participants experienced only one kind of three possible interactions, 45% experienced two kinds of interactions and 41% experienced all three kinds of interactions.

The sample size was determined based on a temporal criterion. Namely we collected data from as many participants we were able to recruit between March 12 and November 30, 2018. Data analysis began only after data collection had been terminated. As exact power

analyses for multilevel designs depend on several assumptions, which we cannot reasonably make due to the lack of empirical data, we performed a simpler post-hoc analysis as described by Ketturat, Frisch, Ullrich, Häusser, van Dick and Mojzisch (2016): The power of detecting within-person effects can be approximated by the power of a correlation test with a sample size that equals the number of participants (e.g., 129) times the number of observations per participant minus 1 (e.g., 8 – 1 = 7). Accordingly, our sample size affords at least 85% statistical power to detect a within-person effect of $r > .1$ assuming two-sided testing.

As compensation, participants received a nine page long individual feedback on their work stress and health behavior during the experience sampling period and entered a price draw for five vouchers worth 50 CHF (approx. 52 USD). If they filled out more than ten experience-sampling questionnaires, they entered additionally a prize draw for an iPad Pro Wi-Fi 11“, 64GB worth 899 CHF (approx. 905 USD).

Procedure

All data collection was done online. Potential participants were informed about key features of the study via a webpage that described the purpose of the study, the procedure, inclusion criteria, the benefit for participants, and gave information on privacy protection. We promoted the study by referring to this webpage via personal contacts, various mailing lists and electronic bulletin boards, alumni organizations of both the University of Zurich and the ETH Zurich, several professional organizations, and by distributing flyers. Interested individuals had to contact the first author/her research assistants to be included. The first author and her research assistants communicated with participants by default with a set of standardized E-mails.

The study consisted of three elements: First, we administered an initial questionnaire that contained questions concerning traits, professional situation, demographic information, and our inclusion criteria (latitude, Work Design Questionnaire, Stegmann et al., 2010; trait self-control, Tangney, Baumeister, & Boone, 2004 among others).

Second, participants took part in an experience-sampling phase for five (if possible consecutive) working days. Each morning at 7 a.m. participants received an E-Mail with two individualized links to the short questionnaire and the evening questionnaire. In this phase, they were instructed to answer a short questionnaire directly after each time they experienced an interaction situation that fit the description already used in Study 1 (real-time, importance, focus on work) with the additional information that the number of interaction partners did not necessarily have to be restricted to one. In the beginning of the short questionnaire, participants selected from a list of possible roles their interaction partner could have had (i.e., situational power). If they interacted with more than one person, they were asked to specify on who they had concentrated most. Then, participants answered the

questions on sense of power, personal power and sense of responsibility. Subsequently, they rated the extent to which they would be able to exert self-control and whether they needed to do so. Finally, participants indicated how stressed they were at the moment and how well they felt able to cope.

Third, after work, we asked participants to answer questions concerning aggregated behavior indicative of self-control over this working day, which will not be reported here. The evening questionnaire also contained most of the information needed for the individual feedback. The order of topics covered in the short questionnaire and the evening questionnaire remained the same during the experience-sampling phase, while the order of items within the topics was randomized.

Measures

Given the time-intensive nature of ES studies, we used very short measures of our focal constructs.

Situational power. Participants reported with whom they had interacted: A subordinate colleague, a colleague on the same hierarchical level, their superior, another superior colleague, a customer, a supplier, or none of the above.

Sense of power. Participants answered the following three questions on a scale ranging from 1 (the other person does) to 5 (I do): “At the moment, who controls more resources that the other needs (e.g., time, money, information)?”, “At the moment, who can make the other do something specific to a greater extent?”, and “At the moment, who can reward and/or punish the other to a greater extent?”. Cronbach’s alpha was .86.

Personal power. Participants indicated to which extent they agreed to the following two items using a scale ranging from 1 (not at all) to 5 (completely): “At the moment, I have the feeling that I can impact on things in my environment” and “At the moment, I have the feeling that I can do things or make decisions without another person is able to interfere”. Cronbach’s alpha was .65.

Sense of responsibility. Participants were asked “At the moment, who feels more responsible for the other?”. They gave their answer on a 5-point scale ranging from 1 (the other person) to 5 (myself).

Start self-control. Participants were asked “If you needed to get going on something right now that you really don’t want to do, how likely is it that you would do it right now?” They indicated the likelihood on a scale ranging from zero to one hundred percent. To clarify what we meant, we gave three examples of situations in which the respective kind of self-control would be needed for every self-control dimension. Additionally, we asked whether their start, stop and/or interpersonal self-control had actually been challenged.

Stop self-control. Participants were asked “If you were facing something you find very tempting right now, how likely is it that you would resist it right now?” They indicated the

likelihood on a scale ranging from zero to one hundred percent.

Interpersonal self-control. Participants were asked “If another person would benefit from you holding back, how likely is it that you would hold back right now?” They indicated the likelihood on a scale ranging from zero to one hundred percent.

Analytic strategy

Multilevel modeling was used to account for the nested data structure resulting from asking the same participants to answer the same questions repeatedly. Analyses were performed using the R packages lme4 (Bates, Maechler, Bolker, & Walker, 2015) and lmerTest (Kuznetsova, Brockhoff, & Christensen, 2017).

The Propositions 2 – 3b and 8 – 10 were tested by evaluating whether the coefficient of the relevant predictor variable was significantly greater (Proposition 9: smaller) than zero. For the variable ”situational power“ we used two dummy variables ”high power“ and ”low power“ with 0 coding for interactions with peers. Accordingly, we expected the coefficient of ”high power“ to be greater than zero and the coefficient ”low power“ to be smaller than zero. For all three self-control dimensions, we included either personal power (cf. Proposition 8 and 9) or sense of responsibility (cf. Proposition 10) as predictors, as well as sense of power in all three analyses. Additionally, the models included the person-specific means of these variables as predictors to explore possible between-person effects (e.g., Hoffman & Stawski, 2009). We started with a random intercept model for all models and subsequently tested the adequacy of adding random slopes using the Bayesian Information Criterion (BIC).

The indirect effects were tested by fitting a two-level structural equation model with random intercepts using the R package lavaan (Rosseel, 2012). We modeled the hypothesized relationships on level 1 and specified a saturated model on level two by adding all variances and covariances to the endogeneous variables.

Results and Discussion

Means, standard deviations, and intercorrelations among the study variables are reported in Table 5.5 (see Appendix). In line with our expectations, the facets of power were significantly positively correlated within individuals. The same was true for the three self-control dimensions. We were surprised to find only one significant correlation between one facet of power (i.e., personal power) and one dimension of self-control (i.e., start self-control).

As a first test, we verified that there was sufficient within-person variability to support multilevel analyses (see Table 5.6) in all dependent variables. With regard to psychological power, our results corroborate findings from the only previous experience sampling study within the power literature (Smith & Hofmann, 2016). In the study by Smith and Hofmann 58% of the variance in psychological power represented situational fluctuations. In our study we split psychological power into sense of power and personal power and found that 72%

and 59% of the variance in subjective power was due to situational fluctuation. This emphasizes the importance of studying power not only as a between-person difference variable, as is common in experimental power literature, but also as a variable that changes substantially within-person.

Table 5.6
Percentage of within-individual and between-individuals variance among variables

Construct	Within-individual variance (e^2)	Between-individuals variance (r^2)	% of within-individual variance
Sense of power	0.89	0.34	72%
Personal power	0.45	0.31	59%
Sense of responsibility	0.85	0.23	79%
Start self-control	333.62	303.49	52%
Stop self-control	422.44	338.42	56%
Interpersonal self-control	319.07	493.49	39%

Note. Given that our measurement occasions are nested within individuals, we first specified a null model to calculate the percentage of within-individual variance for each repeated measures variable. The percentage of variance within-individual was calculated as $e^2/(e^2 + r^2)$.

Confirmatory analyses

Results from our multilevel path analyses are shown in Table 5.7 in the Appendix. All reported multilevel model estimates are unstandardized. Consistent with Proposition 2, participants reported a reduced sense of power in situations in which they had low situational power as compared to peer interactions (γ = -1.00, p < .001), whereas participants reported an increased sense of power in situations in which they had high situational power (γ = 0.99, p < .001). Moreover, as hypothesized in Propositions 3a and 3b, sense of power was positively related to both personal power (γ = 0.14, p < .001) and sense of responsibility (γ = 0.59, p < .001). In line with our expectation, personal power was positively related to start self-control (γ = 2.99, p < .001). Finally, neither the expected negative relationship between personal power and stop self-control (γ = 0.48, n.s.) nor the expected positive relationship between sense of responsibility and interpersonal self-control (γ = -0.39, n.s.) were found. Unexpectedly, we found significant between-person differences. People higher in personal power than the average personal power in the sample were more motivated to employ start as well as stop self-control.

Exploratory analyses

Given the insecurity regarding how many participants we would be able to recruit, we did not include Proposition 1 stating that structural power would determine the probability to have in interactions a certain situation-specific power role (i.e., situational power) in our pregistration. However, Table 5.8 presents an overview on the proportions of low power, peer and high power interactions participants in different structural power positions reported. In line with our expectations, top managers mostly reported being in high power positions,

members of the middle and lower management reported being in all three kinds of situational power positions, and employees mostly reported interacting with peers and superiors.

We tested whether the relationship between situational power and the three self-control dimensions was mediated by sense of power and personal power (start and stop self-control) or sense of responsibility (interpersonal self-control). Tests of the indirect effects revealed that the effect of situational power on start self-control via sense of power and personal power was significant (indirect $effect_{highpower}$ = 0.60, SE = 0.21, 95% CI [0.19,1.02]; indirect $effect_{lowpower}$ = -0.59, SE = 0.20, 95% CI [-0.99, -0.19]. However, the other two indirect effects of situational power on stop and interpersonal self-control were not.

Table 5.8
Proportions of situational power positions as function of structural power

	Structural power				
Situational power	Top Management	Middle Management	Lower Management	Upper non-management	Lower non-management
High power positions	68%	55%	44%	8%	6%
Peer positions	16%	21%	30%	56%	37%
Low power positions	16%	24%	26%	36%	57%

Note. Descriptives reported here are based on 1021 observations from 114 participants: 14 participants did not indicate their structural power, and one only reported interactions with customers that are not considered here.

We also explored the effects of actually facing these self-control challenges in the moment participants completed the short questionnaires. We designed our self-control measures to capture self-control *motivation* rather than actual behavior in order to guarantee that it would be possible to answer these questions in every situation. However, we also asked participants to indicate whether they actually had to exert start, stop, or interpersonal self-control. We added this predictor (0 = no, 1 = yes) to the models presented in Table 5.9 and found it to be significant in all three models. Facing an actual start or interpersonal self-control challenge increased the respective self-control motivation (start: γ = 6.43, p < .001; interpersonal: γ = 6.18, p = .003), whereas the need to actually exert stop self-control reduced stop self-control (γ = -5.82, p = .01).

As shown in Table 5.9, we also investigated the relationships between the dimensions of power (except sense of responsibility because this predictor had no effect in any of the analyses) and both self-control and sense of responsibility that were not part of our hypotheses. Results showed that sense of responsibility was independent of personal power. As in the analysis presented as main hypothesis test, participants in situations in which they experienced higher personal power than their individual average personal power reported higher start self-control (γ = -2.99, p = .01), and there is also a significant between-person effect in the same direction (γ = 7.77, p = .01). However, stop and interpersonal self-control

seem to be determined predominantly by trait-like characteristics. Participants who experienced higher situational power than the average reported more stop self-control (γ = 21.15, p = .01) as well as more interpersonal self-control (γ = 18.74, p = .04). Participants who experienced a higher sense of power than the average reported less stop self-control (γ = -7.40, p = .05) as well as more interpersonal self-control (γ = -9.89, p = .03). We also found that participants who perceived higher personal power than the average reported more stop self-control (γ = 10.67, p < .001). Taken together, both objective and subjective power had independent effects on self-control. These results emphasize the importance of studying power as multifaceted construct.

Table 5.9
Results of exploratory multilevel analyses using situational power, sense of power and personal power to predict sense of responsibility and the three self-control dimensions.

Criterion	Start self-control	Stop self-control	Interpersonal self-control
		Fixed effects	
Parameter			
Intercept	70.57*** (3.77)	63.01*** (3.90)	67.34*** (4.65)
Level 1 (Measurement occasions)			
Low situational power	-0.29 (1.65)	1.64 (1.87)	-1.15 (1.62)
High situational power	-0.79 (2.02)	-3.05 (2.29)	-2.62 (1.97)
Sense of power	-2.02 (0.95)	2.01^{+} (1.07)	0.80 (0.93)
Personal power	3.07*** (0.88)	0.47 (1.00)	0.32 (1.16)
Sense of responsibility	1.49^{+} (0.77)	-0.49 (0.87)	-0.58 (0.75)
Level 2 (Individuals)			
Low situational power	7.10 (8.42)	15.93^{+} (8.75)	-7.17 (10.34)
High situational power	9.96 (7.42)	20.52** (7.73)	19.21* (9.02)
Sense of power	-8.56* (4.04)	-9.41* (4.18)	-7.56 (4.97)
Personal power	8.23** (2.92)	10.94*** (3.00)	4.89 (3.66)
Sense of responsibility	6.55 (3.61)	3.84 (3.74)	-4.55 (4.45)
		Random effects	
Variance			
Intercept	288.17 (16.98)	294.69 (17.17)	483.14 (21.98)
Low situational power	-	-	-
High situational power	-	-	-
Sense of power	-	-	-
Personal power	-	-	44.02 (6.64)
Sense of responsibility	-	-	-
Residual	329.23 (18.14)	421.97 (20.54)	299.83 (17.32)
R^2	33.9%	31.1%	51.9%

Note. For fixed effects standard errors are in parentheses. For random effects standard deviations are in parentheses. (Pseudo-)R^2 was calculated comparing the absolute null model without any predictor to the model presented in this table. N = 128 because one participant who reported only interactions with customers was excluded in these analyses.
*** p < .001. ** p < .01. * p < .05. $^{+}$ p < .10.

In our confirmatory analyses we only included observations that referred to interactions with superiors, peers and subordinates. However, one fifth of our data described interactions with customers and suppliers (for descriptives per role see Table 5.10). Comparing the data from interactions between members of the same organization to interactions with outsiders

revealed two interesting differences on a descriptive level. First, the relationships between our power constructs do not conform to our hypotheses: Although participants in the supplier role report lower levels of sense of power than participants in the customer role, they report higher levels of personal power and sense of responsibility. Second, being in a supplier or customer role has stronger effects on self-control than being in a high power, low power, or peer role.

Table 5.10
Means and standard deviations (in parentheses) of measures per situational power role

	High power *n* = 276	Peer *n* = 464	Low power *n* = 386	Supplier *n* = 255	Customer *n* = 64
Sense of power	4.12 (0.84)	3.08 (0.70)	2.04 (0.79)	3.33 (1.19)	3.89 (1.09)
Personal power	3.74 (0.82)	3.60 (0.88)	3.37 (0.93)	3.72 (0.94)	3.49 (1.05)
Sense of responsibility	4.02 (0.85)	3.26 (0.84)	2.76 (1.04)	4.07 (1.20)	3.19 (1.15)
Start self-control	75.63 (23.82)	74.41 (25.87)	76.55 (24.64)	80.09 (23.90)	69.75 (27.96)
Stop self-control	74.06 (26.59)	71.62 (28.33)	75.32 (26.66)	80.97 (22.95)	70.37 (25.43)
Interpersonal self-control	70.09 (25.95)	67.18 (30.48)	66.34 (30.25)	70.00 (31.84)	56.90 (31.75)

Note. Sense of power, personal power and sense of responsibility were measured on scales ranging from 1 to 5, whereas the three self-control dimensions were measured on scales ranging from 0 to 100.

General Discussion

Experimental research conducted with student participants has documented that *feeling powerful* or *feeling powerless* affects outcomes that have high practical relevance for organizations such as goal pursuit, time perception, advice taking, or self-control (Galinsky et al., 2015; Guinote, 2017). However, it has yet been unclear how results from these studies can be generalized to organizational settings in which individuals have various roles that imply more or less *objective power*. We aimed to fill this gap by presenting a theoretical framework for understanding how objective power in organizations impacts psychological power with its various downstream consequences and empirically testing its key assumptions. We investigated self-control as an example of an organizationally vital outcome. A major strength of our theoretical framework is that it establishes conceptual clarity with regard to the power and self-control constructs that have both been defined broadly and/or inconsistently by disaggregating both constructs into their constituting elements (e.g., see Figure 5.1).

In support of our hypotheses, we found in a preregistered experiment and a

preregistered experience-sampling study that imagining and actually experiencing interactions with superiors (i.e., having low situational power) decreased participants' sense of power as compared to interactions with peers, whereas imagining interacting and actually interacting with subordinates (i.e., having high situational power) resulted in an increased sense of power as compared to interactions with peers. In turn, a higher sense of power was positively related to both personal power and sense of responsibility. These results show that interacting with others belonging to different hierarchical levels is the real-world equivalent of a structural power manipulation in the laboratory (Galinsky et al., 2015).

However, with regard to self-control, the results were complex across the two studies. From a between-person perspective, participants in low power situations (i.e., situational power) reported on a descriptive level both more stop (supporting our hypothesis) and start self-control (contradicting our hypothesis) than participants in high power situations. From a within-person perspective, the effects were inconsistent across the two studies: In Study 1 we found that personal power was negatively related to both start (supporting our hypothesis) and stop self-control (contradicting our hypothesis), whereas in Study 2, as hypothesized, personal power was positively related to start self-control. However, stop as well as interpersonal self-control were unaffected by changing situational circumstances.

The following differences between the two studies might explain these results. First, self-control was measured differently. In Study 1 we rather targeted self-control *performance* and participants answered questions referring to concrete ways of (not) showing self-control. In contrast, in Study 2 we focused on self-control *motivation* and participants answered abstract questions on self-control (see also next paragraph).

Second, given that participants in Study 1 only *imagined* the interactions and based their answers on implicit theories rather than actual experience, we consider the results of Study 2 to be more dependable. Furthermore, the positive within-person effect of power on start self-control complements previous between-person research, unequivocally showing that power is positively related to the concept of initiation on an abstract level (e.g., Keltner et al., 2003; Magee, 2009). On a more concrete level, previous experiments have also shown that participants in high power conditions showed better start self-control performance than participants in low power conditions, e.g., made more attempts to solve difficult tasks (DeWall, Baumeister, Mead, & Vohs, 2011; Guinote, 2007b), were better able to focus on the task at hand (Guinote, 2007c; Slabu, Guinote, & Wilkinson, 2013; Smith, Jostmann, Galinsky, & Van Dijk, 2008), and decided in favor of their long-term interests (Joshi & Fast, 2013). Hence, in the case of start self-control, results from the laboratory seem to generalize to organizational settings. In summary, these inconsistent results emphasize the necessity to complement laboratory experiments with the investigation of experiential episodes.

Unexpected Findings for Effects on Stop Self-Control

Previous experimental research has demonstrated that participants in high power conditions showed worse stop self-control performance than participants in low power conditions, e.g., cheated more (Lammers, Stapel et al., 2010) and did not think their course of action through (Smith et al., 2008; Scholl & Sassenberg, 2015). Fittingly, we found in Study 1 a negative relationship between power and stop self-control performance. However, stop (and also the functionally related interpersonal) self-control motivation were unaffected by power in our Study 2.

This suggests that – at least with regard to stop self-control – it is very important to differentiate carefully between self-control *motivation* and self-control *performance*. Self-control can be considered a process (Kotabe & Hofmann, 2015): Within the activation phase, desire and an at least partly incompatible higher order goal generate desire-goal conflict. This conflict marks the transition to the exertion phase in that it activates control motivation. Control motivation and control capacity interactively determine potential control effort. In the enactment phase, if there are no enactment constraints, the intended behavioral outcome of the previous phase is realized.

Different factors matter in different phases of the (stop) self-control process: At the beginning of the process (i.e., desire strength and conflict), personality has been demonstrated to have a stronger impact than situational factors in down-regulating perceived desire strength and motivational conflict, whereas situational factors showed relatively more influence on components later in the process (resistance and behavior enactment; Hofmann, Baumeister, Förster, & Vohs, 2012). Our Study 2 targeted the beginning of the self-control process. At the same time, our sample scored relatively high on trait self-control (i.e., the mean in our sample, 3.56, was a significantly higher mean than the theoretical scale mean, 3, $t(128) = 11.203$, $p < .001$, $d = 0.99$) due to our study design requiring good self-management skills. As demonstrated in the experience sampling study by Hofmann and colleagues, high self-control participants such as ours might not have experienced motivational conflicts and/or problematic desires and reported constantly high levels of state self-control motivation accordingly. Taken together, this suggests that self-control should be investigated as a multifaceted construct, not only with regard to a functional taxonomy as used here in Studies 1 and 2, but additionally with regard to a process view.

The Role of Responsibility in the Power – Outcome Relationship

Given the recent interest in the role of perceived responsibility (e.g., Sassenberg, Ellemers, Scheepers, Scholl, 2014; Tost, 2015) in effects of power on outcomes, we included sense of responsibility as one dimension of subjective power in our model. In light of our approach to measure the communal outcome of interest *in* the power-relevant situation (namely as behavior directed towards the person with whom the agent has the power

relationship, and not as behavior directed to a person outside the power relationship as it is common in experimental (power priming) research; Tost, 2015), we were surprised by the null effect of sense of responsibility on interpersonal self-control.

In contrast to the studies presented here, the only other experience sampling study in power research fittingly found that feelings of responsibility mediated the relationship between power and the two communal outcomes felt closeness and the wish to interact (Smith & Hofmann, 2015). This discrepancy could be explained in several ways. First, although convincing in theory, sense of responsibility must not necessarily have effects in everyday contexts. Being aware of one's responsibility does not mean that one also acts upon it (Scholl et al., 2017, Study 2). Second, as stated above, the only empirical evidence comes from the study by Smith and Hofmann. In this study the majority of participants were female; one third of the reported situations were classified as family-related and two-third of the family-related situations were high power situations. Taken together, this suggests that mothers with power over their children reported a substantial part of the high power situations. Power over one's children is not necessarily the same as power over subordinates. Third, it might have been a problem of operationalization. Given the time-intensive measurement in Study 2, we confronted participants with a rather abstract question to measure their (interpersonal) self-control, whereas "wanting to interact" or "feeling close" in the study by Smith and Hofmann are more narrowly described behaviors. Fourth, communal behaviors might not be a homogeneous category of behaviors: It is possible that there are subgroups within this broad category. For instance, feelings of closeness or wishes to interact are not tied to normative prescriptions, whereas showing interpersonal self-control is required by norms, especially in a professional context.

Theoretical Implications for Power Research

One of the unsolved conundrums of power research is what might constitute an adequate control condition. Control conditions are necessary to determine whether an effect of power is attributable to power or to powerlessness. The need for control conditions within power research has already been discussed elsewhere (du Plessis, Schaerer, Yap, & Thau, 2016; Magee & Smith, 2013; Moskowitz, 2004). However, these claims have not received a great deal of attention.

Admittedly, in between-person experimental designs, it is rather unattractive to have a control condition. First, conducting 3-cell-design-studies with a control condition is more expensive – more participants need to be recruited, tested, and compensated, which costs more time and money than only comparing conditions in a two-cell design. Second, it is not easy to determine what could constitute an adequate control condition. One idea has been to use non-relational conditions as control conditions (e.g., "write about your day yesterday" within the priming paradigm; e.g., Galinsky et al., 2006); however, power is a *relational*

phenomenon and cannot be meaningfully compared to non-social situations. Another idea is to establish a condition that is equal in terms of access to and control over resources and punishments (i.e., equal structural power). However, participants still might not *perceive* the power balance to be equal, as this perception might depend on many fairly stable individual difference variables, such as trait dominance, interpersonal hierarchy expectation, competence, or one's own standing in the informal hierarchy within one's organization (French & Raven, 1959; Schmid Mast, 2005; Schmid Mast, 2010).

Studying power in within-person designs as we do in the current work provides a solution for the control condition-related problems. On the one hand, within-designs offer a substantial boost in statistical power and require fewer participants than would be needed to conduct a study with a 3-cell-design and adequate power (Charness, Gneezy, & Kuhn, 2012). On the other hand, every individual can be his/her own control condition. The advantage is that the aforementioned factors that influence the perception of the power balance between individuals do not interfere with the interpretation of power's effects, as they are constant across all three conditions. This is the approach we chose in our first study.

However, this "simple within-approach" still relies on the assumption that structural and psychological power are linked by a linear and positive relationship. This is questionable against the background of work by interpersonal circumplex researchers on behavioral complementarity. Complementarity means that people tend to respond to dominant behavior with submissive behavior and to submissive behavior with dominant behavior (Markey, Funder, & Ozer, 2003; Sadler & Woody, 2003). Superiors and subordinates are not primarily their hierarchical roles - they are human beings first. Thus, interacting with a very dominant subordinate might result in a reluctant superior feeling less powerful than the subordinate feels, irrespective of the fact that it should be the other way around given the distribution of structural power.

The more complex within-approach we chose in Study 2 allowed us to circumvent this issue. Sampling multiple interactions an individual has with different people on different hierarchical levels allows for the determination of the individual's baseline psychological power – independently of structural power. Upward or downward deviations from this individual baseline psychological power can then be interpreted as high or low power situations.

Practical Implications

Self-control challenges are prevalent in everyday organizational life. Employees on all hierarchical levels need to concentrate on the task at hand and avoid being distracted by every incoming e-mail. They should neither say or do things in the spur of the moment, nor should they procrastinate important but unpleasant chores. Superiors and subordinates must cope with these demands in order to do their work successfully. Our results suggest that it is

important to differentiate between start and stop self-control because the success in these two kinds of challenges is determined by different factors.

Stop self-control and the closely related interpersonal self-control seem to be determined to a larger extent by interindividual differences. In fields of activity in which impulse control and concentration are imperative (e.g., customer contact, contact with challenging populations such as children or old people), there may be merit in taking these into consideration when recruiting and selecting individuals. In contrast, start self-control is also susceptible to situational influences. This implies that there is a “right moment” to tackle start self-control tasks such as getting started doing something that one dislikes but that needs to be done or revising work that was thought to be completed already. Typically, these self-control challenges might arise following meetings with superiors. As personal power helps in dealing with start self-control challenges, it would be important for superiors to take care to not diminish their subordinates’ personal power in instructing them to do these kinds of tasks.

Limitations and Future Directions

Potential for improvement exists with respect to the empirical testing of our propositions. First, we focused on self-control *motivation* in the current work. Of course, this is only the first step in the self-control process, and various factors can still interfere along the way to self-control *enactment* (cf. Kotabe & Hofmann, 2015). It would have been ideal to sample situations in which participants experienced both a power relevant situation and faced a self-control challenge in order to be able to capture *actual* self-control enactment. However, this would require sampling even more situations to increase the probability of documenting at least some of these kinds of situations. The other possibility would be to put the cart before the horse: One could also ask participants to report situations in which they exerted self-control and what they did before each of these.

Second, we cannot rule out the possibility that the effects of situational power are confounded with effects of specific interaction partners. Participants only reported the interaction partner’s role. However, the same role (i.e., peer or subordinate) can be occupied by different people with different personalities and different relationship history, which probably affects the interaction situation. For instance, if an individual reported four interactions with subordinates in total and all four were interactions with Peter, the effects might either be effects of high power or effects of interacting with Peter. This is less of a problem if an individual’s reported interactions with subordinates referred to interactions with different people.

In contrast to a laboratory environment in which one attempts to minimize the influence of context, context is inescapable in real life (Lewin, 1938). In prior (laboratory) power research, only a limited amount of studies have explicitly taken moderators such as status,

stability or legitimacy into consideration (Schmid Mast, 2010). We encourage further efforts in this direction and suggest previously neglected factors that might qualify the relationship between structural power and psychological power: organizational hierarchy climate, interpersonal hierarchy expectation, self-monitoring, and role integration/segmentation preferences. What these proposed moderators have in common is that they might amplify the perceptions of power differences between hierarchical positions, which should ultimately result in stronger effects on outcomes.

Conclusion

There is a lot of evidence in favor of psychological power as a determinant of various outcomes, either as an explaining mechanism linking structural power to outcomes (e.g., Bombari, Schmid Mast, & Bachmann, 2017; Galinsky et al., 2015) or as a predictor independent of structural power (Smith & Hofmann, 2016). We have demonstrated that psychological power cannot only be manipulated between-person as in previous experimental work, but that there is also substantial within-person variation in people's levels of everyday psychological power. We therefore encourage future research to study power and its outcomes (again) outside the laboratory.

CHAPTER 6: GENERAL DISCUSSION

This last chapter summarizes major findings of this dissertation, provides a reflection on the strengths and limitations of the presented studies and discusses theoretical and practical implications. The main aim of this dissertation was to investigate the effects of social power on self-control as primary operationalization of responsible behavior. Given the broad nature of the definitions of both constructs involved, I have followed a molecular approach in investigating the relationship between objective and subjective power on the independent variable side, and start, stop, and interpersonal self-control on the outcome side.

Does Power Result in Responsible Behavior?

Based on the evidence I have gathered in four experimental studies and one quasi-experimental study (see Table 6.1) using different power manipulations and different outcome operationalizations, the answer is no. Overall, the findings presented in this dissertation revealed no differences in self-control performance (see Chapters 2 and 3) and risk-taking (see Chapter 4) in response to experimental manipulations of power. In contrast, interpersonal accuracy (see Chapter 4) and self-control motivation (see Chapter 5) seem to be affected by power, however the findings were inconsistent across operationalizations or studies, respectively. In the light of these results, reminders of responsibility such as Voltaire's quotation cited in the introduction ("With great power comes great responsibility") remain relevant.

The initial motivation to explore the relationship between social power and self-control arose from conflicting predictions made by the two major power theories, the approach/inhibition theory (Keltner, Gruenfeld, & Anderson, 2003) and the social distance theory (Magee & Smith, 2013). Based on the reinforcement sensitivity theory (RST, Gray, 1991), the approach/inhibition theory suggests that power is related to a heightened attention to rewards and to a drive to experience these rewards immediately, which should ultimately result in poor self-control. Conversely, based on principles of construal level theory (Trope & Liberman, 2010), the social distance theory proposes that the perceived larger social distance to other people experienced by powerholders brings them to engage in higher-level construal, which has been proven beneficial in boosting self-control. Applying a rather broad definition of self-control as the ability to align one's own behavior with superordinate goals, values, and norms (cf. Baumeister, Vohs, & Tice, 2007), evidence exists for both of these mutually exclusive predictions of increased and decreased self-control following the experience of power. To give some examples: Participants in high-power conditions are better able to focus on the task at hand (Guinote, 2007b; Smith, Jostmann, Galinsky, & van Dijk, 2008), and they persist longer and make more attempts to solve (unsolvable) tasks

(Guinote, 2007a) than participants in low-power conditions. They are, however, worse at suppressing thoughts (Guinote, 2007c), withstanding impulses to act (Scholl & Sassenberg, 2015), and they take more risks (Anderson & Galinsky, 2006).

Table 6.1
Overview on characteristics of studies conducted as part of this dissertation

Chapter	Kind of study	Conditions	Sample	Power manipulation	Dependent variable
2	Online Experiment (direct replication)	Between: HP vs. LP vs. C	Students and working adults *n* = 263	Episodic priming	Self-control (behavior): Temporal discounting
3	Laboratory Experiment (conceptual replication)	Between: HP vs. LP	Mostly students, female *n* = 95	Role-play (Interview situation)	Start self-control (behavior): 1. Handgrip task 2. Persistence in problem-solving Stop self-control (behavior): 3. d2 test of attention 4. Emotion suppression
4	Laboratory Experiment	Within: HP vs. LP	Mostly students, male *n* = 78	Role-play (Tangram puzzle task)	1. Risk-taking (behavior) 2. Interpersonal accuracy (behavior)
5	Online Experiment	Within: HP vs. LP vs. C	Non-student *n* = 190	Hierarchical position of imagined interaction partner	1. Start self-control (motivation) 2. Stop self-control (motivation)
5	Experience sampling study	Within: Quasi-experiment, HP vs. LP vs. C	Non-student *n* = 129	Hierarchical position of actual interaction partner	1. Start self-control (motivation) 2. Stop self-control (motivation) 3. Interpersonal self-control (motivation)

Note. HP = high power, LP = low power, C = control

When two theories make conflicting predictions and there is empirical evidence in favor of both, the question arises as to when each theory is right. A first step in answering this question is to establish if previous findings on power's effects on self-control are robust. I conducted two replication studies accordingly. First, the online experiment reported in Chapter 2 was a direct replication of the finding that power would decrease temporal discounting (i.e., improve self-control, Joshi & Fast, 2013, Study 3). Using a much larger sample size but nearly identical procedures to those of the original authors we obtained a

much smaller, non-significant effect. Bolstered by auxiliary analyses, the inability of the power priming paradigm to elevate participants' sense of power seemed to be a reasonable explanation. Second, the laboratory experiment reported in Chapter 3 describes a conceptual replication of previous findings. We developed a more effective role-play power manipulation based on the self-presentation part of the Trier Social Stress Test (Birkett, 2011) and investigated the idea that previously used self-control operationalizations might cluster into two different categories, start and stop self-control, which could be differentially affected by power. However, although the power manipulation strongly affected how powerful/powerless participants felt, we found no effects on self-control performance.

A likely explanation for the two failed replications might be that previous works overestimated the effects of power on self-control. In laboratory settings, neither power nor self-control are operationalized in ways that are consequential for participants, i.e., both constructs lack personal relevance. In investigating the relationship between these constructs in organizational settings where power and self-control actually matter, I tried to address this issue (cf. Chapters 4 and 5). Studying power in the real world implies that there are not only power differences *between* people but that power also varies to a non-negligible extent in rather short time spans *within*-person. However, as there was not much literature I could rely on, I began by investigating power in a within-person laboratory experiment (cf. Chapter 4).

In Chapter 4 I discussed what could be gained by studying power within-person: This approach has methodological advantages such as increased statistical power. It also allows for exploring questions that cannot be answered otherwise, such as whether power gain and power loss are experienced equally strongly or where an individual's "baseline" (psychological) power is situated. The empirical study presented in this chapter showed that it was indeed possible to manipulate psychological power within-person using a newly developed (structural) power manipulation. In order to increase the likelihood of finding effects of this within-person power manipulation I chose outcomes that have been shown to be reliably affected by power manipulations, namely risk-taking and interpersonal accuracy. Both outcomes are related to the idea of responsible behavior. However, although the manipulation affected psychological power, it did not produce the expected effects on the dependent measures.

Finally, Chapter 5 explored the idea of studying power in real life and presented a theoretical framework that links within-person variations in objective power and psychological power to self-control in organizational settings. As before, power and self-control were both conceptualized as multidimensional constructs. The aforementioned experiment and experience sampling study supported the key predictions of the theoretical framework. Variation in objectively demonstrable control of valued resources (i.e., situational power) was

accompanied by variation in subjective power, such that participants experiencing high situational power also reported a high sense of power, high personal power and a high sense of responsibility. However, with regard to the self-control dimensions, a more complex pattern of results emerged. From a *between*-person perspective, participants in low power situations (i.e., situational power) reported both more stop (supporting our hypothesis) and start self-control (contradicting our hypothesis) on a descriptive level than participants in high power situations. This might reflect the larger situational strength in real world settings compared to laboratory settings. In the real world, participants in low power situations might simply lack the freedom necessary to behave in line with their motivational focus, because the consequences of not showing start self-control and stop self-control are equally adverse. From a *within*-person perspective, the effects were inconsistent across the two studies: We found that personal power was negatively related to both start (supporting our hypothesis) and stop self-control (contradicting our hypothesis) in the experiment, whereas in the experience sampling study, as hypothesized, personal power was positively related to start self-control, while stop and interpersonal self-control were not affected by personal power. I consider the results of the experience-sampling study to be more dependable, as these participants actually experienced power. Taken together, the inconsistent results emphasize the necessity to complement experiments with the investigation of experiential episodes.

Strengths and Limitations

I structure my discussion of strengths and limitations according to the four validity types reviewed by Shadish, Cook, and Campbell (2002) in the context of their theory of causal generalization. Of the various threats they named to each validity type, I will comment on the ones relevant to my studies in the next section.

Statistical conclusion validity

Statistical conclusion validity concerns inferences about covariation between treatment and outcome. This implies two related issues, namely whether the presumed treatment and outcome covary and how strongly they do so. There are several reasons why one could be tempted to draw invalid inferences about the existence and magnitude of the covariation between treatment and outcome such as low statistical power, fishing, restriction of range, and unreliability of measures.

First, there is a risk of incorrectly concluding that a relationship between treatment and outcome is not significant if one conducts an insufficiently powered study. Power research in general tends to have rather small samples, although it is likely that the effects of power manipulations (especially power priming) on outcomes are not large (Salomon, 2016). A strength of my dissertation studies is that I consistently considered the issue of power in interpreting my results. I either determined the sample size before data collection (studies

presented in Chapters 2, 3, and 4) or I conducted at least a post-hoc power analysis (studies presented in Chapter 5).

Second, fishing describes the practice of going through the data (e.g., correlate or compare everything with everything) in the hope of finding significant effects and then interpreting these effects as being meaningful. This can lead to false conclusions, given that under null hypothesis significance testing there is the inherent possibility of effects becoming significant “by accident”. Fishing is not an issue in my dissertation because the hypotheses I tested in the five reported studies were preregistered in terms of sample size, analysis method for testing hypotheses, and exclusion criteria. In reporting the results, I clearly characterized results as either confirmatory or exploratory. When I ran multiple tests, I corrected the Type I error rate (see Chapter 3).

Third, unreliable measures might be a problem. A highly reliable measure produces similar results under consistent conditions, so when different conditions yield different results, they likely reflect true differences instead of noise. Unfortunately, some of the outcome measures I used were single-item measures (temporal discounting in Chapter 2, risk-taking lottery task in Chapter 4, self-report measures of start, stop and interpersonal self-control in Chapter 5). However, although single-item scales are psychometrically less favorable than multiple-item scales, they might still be adequate. With regard to the behavioral measures, both paradigms are well-established (risk-taking lottery: e.g., Adam, Kroll, & Teubner, 2014; Bolton, Ockenfels, & Stauf, 2015; Eijkelenboom & Vostroknutov, 2016; temporal discounting: e.g., Curry, Price, & Price, 2008) and empirical evidence points to their construct validity as shown for instance by the findings that individual discount rates predict interindividual variation in real-world health behaviors such as exercise and smoking (Chabris, Laibson, Morris, Schuldt, & Taubinsky, 2008) or that individual discount rates discriminate between pathological and non-pathological gamblers (MacKillop, Anderson, Castelda, Mattson, & Donovick, 2006). With regard to the self-report measures, a clear advantage is that single-item measures do not contain redundancy. It is therefore less boring and frustrating to complete them rather than having to answer a larger number of highly similar questions repeatedly (Gosling, Rentfrow, & Swann, 2003). At the same time, single-item scales can be just as valid as longer scales as shown for instance by the single-item self-esteem scale (SISE; Robins, Hendin, & Trzesniewski, 2001), the single-item measure of social identification (SISI; Postmes, Haslam, & Jans, 2013), or the meta-analysis of single-item measures of overall job satisfaction showing uncorrelated correlations of .63 with scale measures of overall job satisfaction.

Internal validity

Internal validity refers to the adequacy of interpreting a covariation between A, the presumed treatment, and B, the presumed outcome, as a causal relationship. This adequacy

is challenged by ambiguous temporal precedence, systematic differences over conditions in respondent characteristics (selection), attrition, and testing effects among others.

First, in order for the treatment to have an effect on the outcome, the treatment must take place before the outcome is measured. This is usually not the case in correlational studies, however, it is guaranteed in experimental designs. In the experiments I presented in Chapters 2, 3, and 4, temporal precedence is not an issue, as participants first experienced the manipulation before they were confronted with the (clearly separated) outcome measure. In the two studies presented in Chapter 5, however, temporal precedence is not guaranteed. In the experiment, the three levels of the manipulation and the outcome measures were presented at once. Participants were instructed to proceed level by level and give their answers directly in reaction to one level of the manipulation. Despite these instructions, they might have chosen one level as an anchor and given their answer in reference to that anchor in the two other levels. In the experience-sampling study, manipulation and outcome were recorded at the same time. Again, although temporal precedence would be given if participants strictly adhered to the instruction to answer the questionnaires in direct subsequence to the interactions constituting the manipulation, it is not beyond doubt that participants did so. They might have done something else in between the interaction and the completion of the outcome measure or answered the questionnaire with a time lag after the interaction. In these cases, the answers to the outcome measures might reflect something other than the power manipulation. Given that the experiments and the two studies presented in Chapter 5 differ with regard to the conclusion whether power affects self-control, this should be kept in mind (see also conclusion in this chapter).

Second, attrition – the loss of participants in the course of the experiment that is systematically related to experimental conditions – is unlikely to have affected the validity of the presented studies. In the two laboratory studies presented in Chapters 3 and 4, the assignment to conditions took place only when participants arrived in the laboratory and none of the participants failed to complete the outcome measure. The two studies presented in Chapter 5 had a within-person design: In the experiment, participants completed either all three conditions or none. In the experience-sampling study, not every participant reported instances of high-power, low-power, and equal-power; however, this is an inevitable, design-based characteristic of the study. Admittedly, in the online experiment reported in Chapter 2, attrition could have been a problem, as dropout rates differed significantly across the three conditions. However, at least based on gender, age, and professional status the conditions were still similar. It is therefore unlikely that systematic dropout affected the validity of our manipulation.

Third, selection might have compromised our findings reported in Chapter 4. We randomly assigned participants to the power-gain- and the power-loss-condition in this study.

Nonetheless, participants in the power-loss-condition reported less trait risk-taking than participants in the power-gain-condition and this was mirrored in the number of risky choices they made in the experiment. However, in the other studies this was not a problem, as participants were comparable across the experimental conditions in the between-person experiments reported in Chapters 2 and 3, and the studies reported in Chapter 5 had a within-person design that keeps individual differences constant across conditions.

Construct validity

Construct validity concerns the adequacy of inferences about higher order constructs given the measures and manipulations that were used. Construct validity is threatened by an inadequate explication of constructs, the confounding of constructs with levels of constructs as well as reactivity to the experimental situation amongst others.

First, as I have detailed on various occasions throughout this dissertation, both the literatures on power and self-control suffer from the inadequate explication of constructs. Effects of various power manipulations have often been interpreted as effects of high or low power – but what kind of power (e.g., structural or psychological)? Differences in the Stroop task, temporal discounting paradigms, or the success in suppressing emotions have been interpreted as differences in self-control – but what kind of self-control? Initiatory or inhibitory self-control? Self-control capacity or enactment? In my dissertation studies I tried to disaggregate constructs into their smallest possible parts, to define constructs as precisely as possible, and to base my choice of measures on explanations and arguments.

Second, as is common in psychological power research, I did not study power as continuum; hence, the confounding of constructs with levels cannot be ruled out. Usually, power researchers compare high to low to equal power conditions (or only two of these). However, power researchers usually omit no-power and absolute-power conditions. Additionally, it is unclear, whether e.g., high power conditions in two different studies (using different manipulations) are actually comparable. Insights may be gained using a finer gradation such as no-power, low-power, moderate high power (e.g., the real-life equivalent being a team leader), and high power (e.g., the real-life equivalent being a CEO) (cf. Fiedler, 2011). For instance, in the case of self-control, it might be that power decreases stop self-control only in situations of (nearly) absolute power. Initial evidence in favor of following a more fine-grained approach comes from Handgraaf, Van Dijk, Vermunt, Wilke and De Dreu (2008). They found differences between a no-power and a low-power condition. Using a modified ultimatum game, they showed that allocators made low offers to powerless receivers. However, when recipients were completely powerless, offers increased.

Third, participants' behavior not only reflects the objective characteristics of the experimental situation but also participants' subjective perceptions or assumptions about the purpose of the study. I tried to minimize reactivity by deflecting attention from hypotheses

and outcome measures (e.g., by using cover stories, see Chapters 3 and 4). However, a cover story must also be chosen wisely as it can bias participants to behave in a certain way. For instance, the cover story of the experiment reported in Chapter 4 was that I wanted to investigate cooperative work interactions. By providing this anchor, I might have induced participants to avoid making risky decisions. Moreover, in order to be able to control for demand characteristics post-hoc, I asked participants of the laboratory experiments what they thought the respective experiment was about. None of the participants of the experiment reported in Chapter 3 suspected the study's hypotheses. Many of the participants of the experiment reported in Chapter 4 suspected that leadership, power differences, empathy, responsibility or risk-taking was being investigated, but only nine participants formulated ideas for possible research questions in proximity to our actual research questions. Additionally, I standardized experimental procedures and communication with participants as much as possible by means of providing experimenters with a detailed manual (Chapters 3 and 4), providing written information material (Chapter 5), using standardized e-mails to communicate with participants (Chapter 5), and minimizing the number of appointed experimenters.

External validity

External validity describes the adequacy of drawing inferences about whether the treatment-outcome relationship holds across variations in people, settings, manipulations and measures. I tried to recruit diverse samples with regard to age and professional situation (students vs. professionals working in different domains). Students and professionals might differ in their understanding of power, as the former are less familiar with high structural power situations than the latter. Additionally, I studied the relationship between my focal constructs in the laboratory as well as in the field.

However, I chose an all-female sample for the study presented in Chapter 3 and an all-male sample for the study presented in Chapter 4. These decisions were made for practical reasons: Both studies used role-play manipulations and I wanted to avoid possible confounds induced by mixed gender interactions. There were also reasons pertaining to the outcome side: In the case of the study presented in Chapter 3, I wanted to minimize the risk of self-handicapping effects (e.g., in the creative problem solving task), as these effects seem to be less pronounced for women than for men (McCrea, Hirt, & Milner, 2008). In the case of the study presented in Chapter 4, I wanted to increase the probability of having a strong positive effect of power on risk-taking that I could reduce by our manipulation of outcome responsibility. As men are in general less risk averse (Byrnes, Miller, & Schafer, 1999; Cross, Copping, & Campbell, 2011), a male sample seemed to be a good choice. However, given that prior research has documented differences in how men and women substantiate and use their power (Carli, 1999; Eagly, Johannesen-Schmidt, & Van Engen,

2003; Eagly, & Johnson, 1990), results from power studies should not be generalized across gender.

Finally, I studied well-educated samples. I either *know* that participants in these samples scored high on trait self-control (Chapter 5, experience sampling study) and rather low on trait risk-taking (Study in Chapter 4) or I *assume* that they are good at controlling themselves (studies in Chapters 2 and 3), because trait self-control is positively related to a higher grade point average (Tangney, Baumeister, & Boone, 2004), which is a precondition for attending a university or completing a qualified education. Trait self-control has been shown to affect how individuals tackle state self-control challenges (e.g., Baumeister, Wright, & Carreon , 2018; de Ridder, Kroese, & Gillebaart, 2018; Forestier, Sarrazin, Allenet, Gauchet, Heuzé, & Chalabaev, 2018). With regard to the outcome self-control, it is therefore not necessarily the case that my results would have been the same had I studied other samples with more variation in trait self-control or lower trait self-control in general as power might have more room to have its effects in participants low(er) in trait self-control.

Conclusion

Taken together, this dissertation has some notable strengths: First I respected open science best practices in all studies by preregistering my main hypotheses and making materials and data publicly available via the Open Science Framework. Second, I made an effort to establish good construct validity: I circumvented mono-operation- and mono-method-bias by using multiple operationalizations. I manipulated power by means of episodic priming (Chapter 2), I developed two different role-play power manipulations (Chapters 3 and 4), and I investigated a “situational power manipulation” online and in everyday life (Chapter 5). Likewise, I used different approaches to measure self-control – a set of well-established laboratory measures (Chapters 2 and 3) and two different self-report measures (Chapter 5). Third, I addressed the lack of research on power in natural contexts by conducting research both in the laboratory (Chapters 2 – 4) and in organizational settings (Chapter 5) with student as well as employed participants.

However, in addition to the threats to validity discussed in the previous sections (e.g., single-item measures, ambiguous temporal precedence in the studies presented in Chapter 5, confounding of constructs with levels), the following issue may warrant consideration. The laboratory experiments with mostly student participants presented in Chapters 2 and 3 found no effect of power on self-control *performance*. In contrast, the two studies with employed participants presented in Chapter 5 yielded non-causal evidence for an effect of power on self-control *motivation*. However, as the measures and manipulations used, the levels of investigation (between vs. within), and the samples studied were different between these two groups of studies it is not possible to (empirically) tell what caused this difference in findings. More systematic research that varies only one parameter and keeps all other parameters

constant is needed to identify the critical factor responsible for the differences.

Implications for Theory and Future Research

As shown in the previous section, the research presented in this dissertation allowed me to draw mostly valid conclusions about the absence of causal effects and the rather small size of non-causal effects of power on self-control. Together with the work by Zhang and Smith (2018), the studies presented as part of this dissertation are the only preregistered studies on the relationship between power and self-control. These studies have corrected the public record in that they produced null findings while previous research did find effects of power on self-control. Future research will have to acknowledge that certain assumptions are not empirically true, or at least much harder to detect and therefore probably of lesser relevance. In the following I discuss why my results are a valuable contribution and I also discuss the very promising strategy of identifying effects of power with real-world implications I presented in Chapters 4 and 5.

Striving for Conceptual Clarity in Power Research

The power construct suffers from a lack of conceptual clarity. Constructs defined as broadly as power (and self-control) make it difficult to understand related phenomena because different operationalizations that accentuate different aspects of these broad definitions might result in seemingly conflicting findings (for example see Table 3.2). The two major power theories, the approach/inhibition theory (Keltner et al., 2003) and the social distance theory (Magee & Smith, 2013) do not elaborate on their conceptualization of power. The approach/inhibition theory defines power as “an individual’s relative capacity to modify others’ states…” (Keltner et al., 2003, p. 265) and additionally specifies power bases by adding “…by providing or withholding resources or administering punishments” (p. 265; cf. coercive and reward power, French & Raven, 1959). In comparison, the social distance theory defines power as “asymmetric control over valued resources” (Magee & Smith, 2013, p. 159) without specifying power bases and stresses that power is a dyadic concept (cf. principle 1). Both theories understand power in terms of a structural characteristic and consider power to be more of a between-person difference variable, as possible within-person variation is never mentioned. However, neither of the two provide guidance on how to operationalize the abstract construct of power or explain in detail how objective structural power is translated into the *psychological experience of power*. The latter issue is addressed only in the theoretical framework by Tost (2015). She suggests that structural power could manifest psychologically in two forms. The first psychological manifestation is the conscious *sense of power*, namely “one’s evaluation of the extent to which one has the ability to influence others” (Tost, 2015, p. 35). The second is the mostly subconscious *cognitive network of power* that consists of learned associations between power and various cognitive,

affective, and behavioral tendencies. Tost's framework has been a very important first step in understanding how structural power is linked to psychological power and organizational outcomes. Unfortunately, it is not precise enough to be used as basis of empirical research in organizational settings.

The framework I presented in Chapter 5 is meant to extend Tost's (2015) ideas and make them testable. In my framework, I tried to disaggregate power into the narrowest definable units possible: I split *social power* (i.e., the ability to influence others' behavior due to asymmetrical control over valued resources; Fiske & Dépret, 1996; Keltner et al., 2003) into *structural* power (i.e., the objectively demonstrable control of valued resources; Tost, 2015), *situational* power (i.e., the superiority of the own or the other's stable hierarchical position within a specific social interaction), *sense of power* (i.e., the subjective judgment about one's own ability to influence others in a given social situation), *personal power* (i.e., the subjective judgment about one's own competence, agency, autonomy, and independence; cf. Overbeck & Park, 2001; Van Dijke & Poppe, 2006; Lammers, Stoker, & Stapel, 2009) and the *sense of responsibility* (i.e., a state of being concerned for others' well-being and aware of the consequences of one's own actions for others; Tost, 2015). In doing so, I also separate objective power (i.e., structural and situational power) from psychological power (i.e., sense of power, personal power, sense of responsibility; see Chapter 5) and acknowledge that psychological power might have both a variable and a trait-like component (see Chapter 4).

The two major differences between Tost's framework and mine are that I (1) introduced an additional step between structural power and sense of power and (2) conceptualized the cognitive network of power differently. First, I proposed that situational power, namely the interaction-specific distribution of structural power, is the link between stable structural power on the between-person level as expressed by a position in an organizational chart and sense of power as psychological representation of power. This additional step is necessary in order to accommodate the fact that power in real world (organizational) settings is dynamic in the sense that a given person is likely to experience variations in structural power throughout the day. For instance, members of the middle management are subordinates when interacting with their bosses, but they are superiors when interacting with their subordinates. As demonstrated in the experience-sampling study (see Chapter 5) and the only other experience-sampling study in this domain (Smith & Hofmann, 2016) the within-person variance in (structural) power is substantial (> 50 %) and cannot be neglected.

Second, I replaced the cognitive network of power postulated by Tost (2015) with the construct of personal power. Tost assumes that this network is composed of constructs related to agency. Agency describes people's experience of being capable of producing their intended effects in the environment and acting without interference from others. The

cognitive network of power is thought to operate both subconsciously and to a lesser extent also consciously. Although bolstered by findings in laboratory studies showing that power is in fact able to affect behavior subconsciously (for a review see Smith & Galinsky, 2010), investigating subconscious effects of power in real world settings is not feasible. Whereas an individual's environment can be controlled in laboratory settings to the extent that changes in behavior can be attributed to the presence or absence of subconscious manipulations with satisfactory probability, there are too many possible cues in real world settings to draw inferences about causal relationships between subconscious cues and behavior. I therefore decided to include personal power as conscious judgment about one's own agency in my framework instead of the more subconscious cognitive network of power.

My approach to discern the power dimensions listed above also speaks to the issue of operationalization. Different laboratory power manipulations used in previous research target different power dimensions. For instance, economic games such as dictator games can operationalize structural power, but semantic priming cannot. Notwithstanding, researchers have often interpreted their findings in terms of "(a lack of) power resulted in effect X", but they did not articulate which dimension of power (see above) likely caused the effect. The few exceptions (Georgesen & Harris 2000; Smith & Hofmann, 2016; Strelan, Weick, & Vasiljevic, 2014; studies presented in Chapter 5) investigating multiple power dimensions in the same study demonstrated that they have independent and also interactive effects. For instance, findings from an experience-sampling study showed that structural power and feelings of power had significant independent effects on happiness, mood, stress, perceived control, independence, felt closeness to others, as well as the wish to interact with others (Smith & Hofmann, 2016). In another study, chronically powerless individuals sought more revenge than chronically powerful individuals following various situational manipulations of power (Strelan et al., 2014).

The increase in definitional precision could help researchers make sense of inconsistent findings. This applies, for instance, to the example of communal dependent variables (e.g., perspective taking, objectification, interpersonal sensitivity). As discussed in more detail by Tost (2015), there is evidence suggesting both a positive effect of power on communal dependent variables as well as a negative effect. However, the direction of the effect might be a function of the power manipulation used: the vast majority of studies finding a negative effect used episodic priming, which directly targets psychological power (Galinsky Magee, Inesi, & Gruenfeld, 2006; Gruenfeld, Inesi, Magee, & Galinsky, 2008; Lammers, Galinsky, Gordijn, & Otten, 2012). In contrast, the majority of studies reporting a positive effect of power used role-play manipulations, which are designed to reproduce differences in structural power and might make the powerholder's responsibility more salient accordingly (Schmid Mast, Jonas, & Hall, 2009; Overbeck & Park, 2001; Overbeck & Park, 2006; Tost,

Wade-Benzoni, & Johnson, 2015). To conclude, I encourage future research to continue the study of power on a molecular level as well as to investigate within-person variations in power more thoroughly and identify conditions under which the effects are similar or different to those established in between-person designs.

Striving for Conceptual Clarity in Self-Control Research

In a similar vein to the power construct, I proposed to disaggregate self-control based on the content of self-control challenges in start (i.e., self-control that is needed for initiating goal-directed behavior), stop (i.e., self-control needed for suppressing behavior or refraining from acting impulsively), and interpersonal self-control (i.e., self-control needed to maintain good interpersonal relations). The few previous studies that differentiated these dimensions of self-control showed that trait start and stop self-control differed in their ability to predict health behaviors (Allom & Mullan, 2014), contextual performance at the workplace (de Boer, van Hoft, & Bakker, 2015), and on a societal level behaviors such as homicide, suicide, home foreclosures, divorce, and infidelity (Findley & Brown, 2017). I attempted to translate these content-related trait self-control dimensions into state self-control motivation (see Chapter 5) and state self-control performance (see Chapter 3). I found that self-control motivation was differentially affected by dimensions of power; however, self-control performance was not. This does not necessarily disconfirm the hypothesis that different dimensions of self-control are differentially affected by power, as testing a hypothesis requires making additional assumptions regarding operationalization (i.e., the laboratory measures I chose represent start and stop self-control well), which can be wrong.

State self-control research as a whole suffers from pronounced operationalization issues. Two kinds of observations raise the question of what the various performance tasks used within this domain actually measure. First, there are objections with regard to the variety of self-control measures used in laboratory settings. The correlations within and between typically used tasks such as delay of gratification/delay discounting tasks or tasks targeting executive functions (e.g., Go/No-Go task, Stroop task) are rather low (r = .11 to r = .21; Duckworth & Kern, 2011), which does not inspire confidence in the claim that they all measure the same construct. In addition, the selection of tasks meant to measure self-control performance is rarely justified in terms of why exactly the authors think that performance in this task speaks to self-control and (new) tasks are rarely empirically validated (Lurquin, 2017). Furthermore, there are cases of measures that have not only been used to operationalize self-control but also other potentially related constructs. For instance, the Stroop task has been used to measure both self-control (e.g., Gailliot, Plant, Butz, & Baumeister, 2007, Study 3) and executive functions (e.g., Inzlicht & Gutsell, 2007), as well as delay discounting has been used to operationalize both self-control (Hardisty & Weber, 2009) and impulsivity (e.g., Bickel, Odum, & Madden, 1999).

Second, an even more polemical objection is the question whether self-control *must be* measured outside the laboratory. The commonly used laboratory tasks are not so much about dilemmas that are actually meaningful for individuals (de Ridder et al., 2018). People very likely differ in their exertion of self-control in accordance with the personal importance of a goal. For instance, people might try much harder to lose weight by exercising in order to look good in swimwear or to study for a professional training exam that enables a promotion, than to squeeze the handles of a handgrip longer or better concentrate on cognitive tasks such as the Stroop or the Flanker task. Exercising or learning in these examples have long-term consequences such as better health and self-confidence or more money and responsibility. Squeezing handgrips or concentrating have no consequences at all, in most cases not even on the compensation in the experiment. Therefore, it could be that typical laboratory self-control tasks measure differences in the degree to comply with the experimenter's instructions. In summary, given these criticisms, it might be worth exploring this content-based taxonomy of self-control within power research using everyday behavioral indicators of self-control performance.

How to Study the Relationship Between Power and Self-Control

The two major power theories, the approach/inhibition theory (Keltner et al., 2003) and the social distance theory (Magee & Smith, 2013) differ in how they conceptualize self-control. The approach/inhibition theory does not use the term *self-control* but refers to *disinhibition*, a state characterized by reward-focused, need-consistent, and potentially counternormative behavior. In spite of these different terms, the description of disinhibition fits the more inclusive overall definition of self-control used in this dissertation – self-control as the capacity to align one's behavior with different kinds of standards and long-term goals (Baumeister, Vohs, & Tice, 2007) – in that disinhibition is the absence of self-control. With regard to the more fine-grained distinction in start and stop self-control that was proposed in this dissertation, disinhibition seems to be related to stop self-control. Thus, the approach/inhibition theory can be read as indicating that power impairs stop self-control due to the powerfuls' heightened attention to rewards and their drive to experience these rewards immediately.

Additionally, the approach/inhibition theory postulates that power leads to approach-related behavior by means of the power-immanent activation of the behavioral approach system (BAS). Keltner and colleagues (2003) explicitly referred to approach-related behavior in the domains of eating, offensive aggression, and sexual behavior. However, this approach tendency seems to be present in other domains as well, for instance in goal striving (e.g., Galinsky, Gruenfeld, & Magee, 2003; Guinote, 2007b) and motor behavior (e.g., Maner, Kaschak, & Jones, 2010; Smith & Bargh, 2008). As start self-control, i.e., the initiation of goal-consistent action despite a default tendency toward inaction, is also an example of

approach-related behavior, power could be expected to benefit start self-control. In summary, the approach/inhibition theory would benefit from incorporating the content-based distinction within the self-control construct as this would allow for the formulation of more nuanced predictions.

In comparison, the social distance theory is narrower in focus. In this theory, self-control is said to be exerted when "individuals prioritize more important, longer term motives over less important, shorter term motives when the two motives directly conflict" (Magee & Smith, 2013, p. 167). The authors assume that self-control consists of inhibiting automatic thoughts or impulsive responses connected to these short-term motives in order to shield the long-term outcome from hindrances. The theory predicts that power is positively associated with self-control due to the powerfuls' use of high-level construal of goals and situations. This conflict-based conceptualization fits neither my definition of start self-control nor of stop self-control well, as both dimensions are necessary to prioritize long-term goals over short-term goals. For instance, if you want to lose weight in the long run it is necessary to bring yourself to start doing physical training (start self-control) as well as to keep yourself from snacking on sweets (stop self-control). In this conflict-based self-control conceptualization, it is not possible to classify a self-control challenge as either requiring start or stop self-control from the outside, because individuals might differ in their habitual or spontaneous opinion (about the same kind of self-control challenge) of whether their focus is on initiating goal-directed action or on refraining from acting impulsively. To return to the example given above: Peter and Mary might both want to lose weight. Whereas Mary finds it rather easy to exercise but struggles not to snack on sweets, Peter does not like to move at all but has fewer problems abstaining from sweets. Losing weight might be a stop self-control challenge for Mary while it might be a start self-control challenge for Peter. This implies that it would be necessary to ask participants for their personal judgment of what kind of self-control they perceive they predominantly need in order to face a certain self-control challenge.

Given that the self-control definition used in the social distance theory is not compatible with the content-based self-control conceptualization I proposed, it might make sense to study self-control from the perspective of the social distance theory using another possibility of disaggregating the broad self-control construct; self-control can also be considered a process with the three stages activation, exertion, and enactment (integrative self-control theory, Kotabe & Hofmann, 2015). In the activation stage, an individual experiences a desire that is at least to a certain extent in conflict with a higher order goal. In the exertion stage, control motivation and control capacity (i.e., the non-motivational cognitive resources one has available in a given moment to override desire with a higher order goal) interactively determine potential control effort. In the enactment stage, the intended behavioral outcome of the previous phase is realized if there are no enactment constraints. Previous studies have

focused on the effects of power on self-control capacity and self-control enactment. Laboratory findings suggest the trend that people in high power conditions have a better self-control capacity (e.g., Schmid, Kleiman, & Amodio, 2015; Slabu, Guinote, & Wilkinson, 2013; Smith, Jostmann, Galinsky, & van Dijk, 2008), but are not necessarily better in the enactment phase (e.g., HP > LP: Tost, Wade-Benzoni, & Johnson, 2015 vs. HP < LP: Lammers, Stapel, & Galinsky, 2010 vs. HP = LP: Zhang & Smith, 2018). Empirical evidence with regard to self-control motivation is limited. One article found that power benefits self-control motivation/performance if the task is perceived as worthy of leaders (DeWall, Baumeister, Mead, & Vohs, 2011). The studies presented in Chapter 5 suggest that start self-control motivation is predicted by differences in power within individuals and all three self-control motivation dimensions are also predicted by differences in power between individuals. How power impacts on self-control activation has not been investigated yet. Future research could investigate the so far understudied stages.

Finally, in line with the prevalent implicit assumption within power research that power operates in a linear fashion I hypothesized and tested linear (between-person) effects but failed to find them. However, this assumption may have to be challenged and a curvilinear relationship between power and self-control could be tested instead. High power and low power could both be related to higher self-control than equal power albeit for different reasons: When among peers (i.e., in the equal power condition) people might not perceive the need to present themselves as particularly disciplined or persistent. In contrast, subordinates might think that they need to meet or exceed their superiors' expectations either in order to be rewarded or out of fear of negative consequences if they do not. Superiors might be motivated to act and decide in a self-controlled way by organizational properties such as informal dependencies or alignment of goals, because this behavior will probably be rewarded by subordinates with good work and loyalty. To answer this question, it would be necessary to have studies with three-cell-designs, i.e., studies including a low power, high power, and a control condition as data basis. However, three-cell designs are in the minority within the power literature (du Plessis, Schaerer, Yap, & Thau, 2016), and the specific part of this literature that investigates the relationship between power and self-control is no exception (see Table 3.2). Future research might therefore also test for curvilinear effects of power.

Other Operationalizations of Responsible Behavior

This dissertation investigated self-control as the main operationalization of responsible behavior. Although a rather broad conceptualization of self-control was used, there are still many other measures that capture (parts of) defining characteristics of responsible behavior. For instance, possible outcome operationalizations could be cognitive perspective taking (e.g., Oswald, 1996), prefactual thinking (e.g., Sanna, 1996), (future-oriented) time

perspective (Zimbardo, Keough, & Boyd, 1997), or decision-making in all kinds of situations marked by a conflict between interests of different stakeholders, between the self and others, or present vs. future interests as such present in discounting paradigms combining social with temporal or probability discounting (e.g., Charlton, Yi, Porter, Carter, Bickel, & Rachlin, 2013), or decision making for others (e.g., Pahlke, Strasser, & Vieider, 2015). These outcomes have received only limited attention within power research to date (Galinsky et al., 2006; Scholl & Sassenberg, 2015) but would be relevant for organizational settings as well.

Practical Implications

Responsible superiors would not focus solely on their own interest, but at least get a general idea of the interests of all stakeholders involved in a decision, be it internal stakeholders such as subordinates, their own team or organization as a whole, or external stakeholders. They will of course face conflicting interests, but the responsible superior would try to find a fair solution that minimizes negative impact on the affected parties and consider long-term consequences. Both from a normative point of view and from the point of view of the stakeholders, a responsible superior would be ideal.

However, based on the results presented in this dissertation it seems that, on a situational level (i.e., single interactions in laboratory settings or interactions at the workplace), power and responsible behavior are not systematically related (cf. null findings in Chapters 2 and 3 but non-causal evidence for relationships between power and self-control in Chapter 5) – even though the responsibility for subordinates should be obvious when one is in a position with high structural power because this position grants the possibility to do good or avoid harm to a larger extent than the subordinate position does. My results also show that this responsibility is not reflected in outcomes even when it is perceived.

If responsible leader *behavior* is less a question of momentary perceptions but more a question of stable factors such as personality, there might be merit in selecting or promoting the “right” people to positions of power. Apart from the role-based responsibility addressed above, i.e., responsibility given to an individual by others higher up in hierarchy (cf. accountability, Hall, Royle, Brymer, Perreweé, Ferris, & Hochwarter, 2006), responsibility can also be a trait-like inner obligation to take care of other people (completely independent of power conceptually) that is likely to stem from a stable tendency to focus on other people. A multitude of concepts describes this tendency such as interdependent self-construal (Aron, Aron, & Smollan, 1992; Cross, Bacon, & Morris, 2000; Howard, Gardner, & Thompson, 2007), empathy (Davis, 1983), concern for others (Ravlin & Meglino, 1987), social value orientation (Van Lange, 1999) and interpersonal values (Locke, 2000). People high in this tendency are likely to be the more responsible powerholders (Sassenberg, Ellemers, Scheepers, & Scholl, 2014; Tost, 2015; Williams, 2014).

Conclusion

Both power and self-control have received a lot of attention within social psychological research (and beyond) during the last decades. Research with either of both of these constructs must still tackle important issues such as establishing conceptual clarity (both) or making the step from the laboratory to the real world (power). This dissertation tried to contribute a piece to the puzzle by disaggregating the broad constructs of self-control and power into their molecules and by proposing a theoretical framework for how to study power in organizational settings. In conclusion, future power research might build on the insight into how objective, structural power is translated into outcomes via perceptions of psychological power. The highly promising route of studying the effects of power using the contextual approach presented in this dissertation should be chosen to investigate outcomes other than self-control in order to complement previous between-person laboratory research.

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APPENDIX

Table 1.1
Overview on studies on the relationship between power and self-control.

Source	Study	Research Design	Sample	Level of analysis	Kind of power	Self-control operationalization	Relevance of power	Relevance of self-control
DeWall et al. (2011)	1A	Laboratory experiment	Student	Between	Sense of power	Dichotic listening task	no	no
DeWall et al. (2011)	1B	Laboratory experiment	Student	Between	Sense of power	Number of attempts to solve solvable anagrams	no	no
DeWall et al. (2011)	2	Laboratory experiment	Student	Between	Sense of power	Dichotic listening task	no	no
Grossklags & Barradale (2014)		Laboratory experiment	Student	Between	Structural power	Hypothetical monetary temporal discounting	no	no
Guinote (2007a)	1	Laboratory experiment	Student	Between	Sense of power	Thought suppression task	no	no
Guinote (2007b)	1	Laboratory experiment	Student	Between	Sense of power	Action planning in hypothetical situations: time and information needed	no	no
Guinote (2007b)	2	Laboratory experiment	Student	Between	Sense of power	Time needed to initiate goal-directed action	no	no
Guinote (2007b)	3	Laboratory experiment	Student	Between	Sense of power	Problem-solving task (Drawing a geometric figure without retracing any lines and without lifting the pencil from the paper)	no	no
Guinote (2007c)	1	Laboratory experiment	Non-Student	Between	Sense of power	Framed-line test: inhibition of peripheral information	no	no
Heller & Ullrich (2017)	-	Online experiment	Adult convenience sample	Between	Sense of power	Hypothetical non-monetary temporal discounting	no	no
Joshi & Fast (2013)	1	Online experiment	Mturk	Between	Sense of power	Hypothetical monetary temporal discounting	no	no

Table 1.1 cont.
Overview on studies on the relationship between power and self-control.

Source	Study	Research Design	Sample	Level of analysis	Kind of power	Self-control operationalization	Relevance of power	Relevance of self-control
Joshi & Fast (2013)	2	Online experiment	Student	Between	Sense of power	Semi-hypothetical monetary temporal discounting	no	no
Joshi & Fast (2013)	3	Online experiment	Student	Between	Sense of power	Hypothetical non-monetary temporal discounting	no	no
Joshi & Fast (2013)	4	Correlational online study	Working individuals (Mturk)	Between	Self-reported power	Lifetime savings	yes	yes
Lammers et al. (2010)	1	Laboratory experiment	Student	Between	Sense of power	Actual cheating behavior	no	(yes)
Li et al. (2016)	-	Laboratory experiment	Student	Within	Sense of power	Anger expression tendency	no	(yes)
Petkanopoulou et al. (2012)	1	Correlational questionnaire study	Adult convenience sample (Airport)	Between	Sense of power	Emotional Regulation Questionnaire (Gross & John, 2003)	(yes)	(yes)
Petkanopoulou et al. (2012)	2	Laboratory experiment	Student	Between	Sense of power	Emotion Suppression (negative-emotions eliciting pictures)	no	no
Schmid et al. (2015)	1	Laboratory experiment	Student	Between	Sense of power	Flanker Task	no	no
Schmid et al. (2015)	2	Laboratory experiment	Student	Between	Sense of power	Flanker Task	no	no
Scholl & Sassenberg (2014b)	1	Laboratory experiment	Student	Between	Sense of power	Time spent on thinking before starting to type an email	no	no
Scholl & Sassenberg (2015)	1	Laboratory experiment	Student	Between	Sense of power	Number of prefactual thoughts	no	no
Scholl & Sassenberg (2015)	2	Laboratory experiment	Student	Between	Sense of power	Number of prefactual thoughts	no	no

Table 1.1 cont.
Overview on studies on the relationship between power and self-control.

Source	Study	Research Design	Sample	Level of analysis	Kind of power	Self-control operationalization	Relevance of power	Relevance of self-control
Slabu et al. (2013)	-	Laboratory experiment	Student	Between	Structural power	Spatial cueing task: response time on invalid trials	(yes)	no
Smith et al. (2008)	2	Laboratory experiment	Student	Between	Sense of power	Stroop task: error rate	no	no
Smith et al. (2008)	3	Laboratory experiment	Student	Between	Sense of power	Tower of Hanoi task: error rate	no	no
Smith et al. (2008)	4	Laboratory experiment	Student	Between	Sense of power	Stroop task: error rate	no	no
Yap et al. (2013)	1	Field experiment	Convenience Sample (Train station)	Between	Cognitive power network	Actual cheating behavior	no	(yes)
Yap et al. (2013)	2	Laboratory experiment	Student	Between	Cognitive power network	Actual cheating behavior	no	(yes)
Yap et al. (2013)	3	Laboratory experiment	Student	Between	Cognitive power network	Actual cheating behavior	no	(yes)

Table 3.1
Overview of conceptualizations of multidimensional self-control/impulsivity

Domain	Authors	Organizing principle	Dimensions and measures						
			I	II	III	IV	V	VI	VII
Self-report measures	De Boer, Van Hooft, & Bakker (2011)	Function	Start self-control *Items targeting control needed to do something that a person would otherwise not do*	Stop self-control *Items targeting control needed to not do something that a person would otherwise do*					
	De Ridder, de Boer, Lugtig, Bakker, & van Hooft (2011)	Function	Initiation *Items targeting activation of long-term goal*	Inhibition *Items targeting inhibition of indulgence*					
	Maloney, Grawitch, & Barber (2012)	Function	Impulsivity *Items targeting action on spontaneous thoughts and feelings*	Restraint *Items targeting the tendency to resist temptation*					

Table 3.1 cont.
Overview of conceptualizations of multidimensional self-control/impulsivity

Domain	Authors	Organizing principle	Dimensions and measures						
			I	II	III	IV	V	VI	VII
Behavioral measures	Hagger, Wood, Stiff, & Chatzisarantis (2010)	Modality	Cognitive processing	Affective processing					
	Hagger, Wood, Stiff, & Chatzisarantis (2010)	Task content	Controlling attention* - *Computerized vigilance task*	Controlling emotions* - *Regulate affect while presented with emotive video or pictures*	Controlling thoughts* - *Suppressing thoughts of a 'white bear'*	Controlling impulses* - *Handgrip task* - *Modified Stroop task* - *Unsolvable geometric figure-tracing task* - *Persistence on cold pressor task*	Cognitive processing* - *Solvable anagram/ word fragments task* - *Analytical reasoning GRE problems* - *Working memory tasks*	Choice and volition* - *Consumer decision task* - *Choosing to solve a challenging or easy anagram task* - *Temporal discounting*	Social processing* - *Test of stereotyping and prejudice based on ratings of target* - *Perceived likelihood of engaging in sexual infidelity*
	Sharma, Markon, & Clark (2014)	Task content	Inattention - *Stroop (error)* - *Delayed memory* - *Immediate memory* - *Porteus Maze Test*	Inhibition - *Go/No Go (inhibition)* - *MFFT (latency)* - *Circle tracing task (slow time)* - *MFFT (error)* - *Iowa gambling task* - *Stop-signal reaction time*	Impulsive decision-making - *Delay of gratification (hypothetic)* - *Delay of gratification (contingent)*	Shifting - *Wisconsin Card Sort Task*			

Table 3.1 cont.
Overview of conceptualizations of multidimensional self-control/impulsivity

Domain	Authors	Organizing principle	Dimensions and measures						
			I	II	III	IV	V	VI	VII
Self-report & behavioral measures	Duckworth & Kern (2011)	Measurement & Task content	Self-report - *Eysenck Impulsiveness Scale* - *Self-Control Scale* - *Barratt Impulsiveness Scale*	Delay of gratification/ temporal discounting - *Hypothetical choice delay task* - *Real choice delay task* - *Sustained delay task* - *Repeated trials delay task*	Executive functioning - *Go/No-go Task* - *Stroop task* - *Set switching tasks* - *Attention tasks* - *Gambling tasks* - *Risk tasks*	Informant report *No information available*			
	Lane, Cherek, Rhoades, Pietras, & Tcheremissine (2003)	Measurement & Task content	Self-report - *Barratt Impulsivity Scale* - *Eysenck Impulsiveness Questionnaire* - *Dickman Impulsivity Inventory* - *Wender Utah Rating Scale*	Delay of reward - *Self-Control Choice Task* - *Hypothetical delay discounting* - *Contingent delay discounting*	Response inhibition - *Immediate Memory Task* - *Response Inhibition/ DRL Task*				

Table 3.1 cont.
Overview of conceptualizations of multidimensional self-control/impulsivity

Domain	Authors	Organizing principle	Dimensions and measures						
			I	II	III	IV	V	VI	VII
Self-report & behavioral measures	Reynolds, Ortengren, Richards, & de Wit (2006)	Measurement & Task content	Self-report - *Barratt Impulsive-ness Scale* - *Eysenck Impulsive-ness Question-naire* - *Multidimens. Personality Question-naire (constraint scale)*	Impulsive decision-making - *Delay discounting* - *Balloon analogue risk task*	Impulsive disinhibition - *Stop task* - *Go/no-go task*				
	Reynolds, Penfold, & Patak (2008)	Measurement & Task content	Self-report - *Barratt Impulsivenes s Scale* - *Conners-Wells' Adolescent Self-Report Scale*	Impulsive decision-making - *Delay discounting* - *Probability discounting* - *Experiential discounting*	Impulsive disinhibition - *Experiential discounting latency to bank rewards* - *Conners' Continuous Performance Test-II*	Impulsive inattention - *Go/stop task*			

Note. *Only examples shown.

Table 3.2
Studies on power and self-control classified according to inhibitory and initiatory self-control

Source	Study	Power manipulation	Dependent measure (self-control operationalization)	Main result	HP = more self-control?	Functional component
DeWall et al. (2011)	1B	Episodic priming (HP, LP, C)	Number of attempts to solve solvable anagrams	LP = C = HP	Equal	Start
DeWall et al. (2011)	1A	Not enacted role assignment (HP, LP, C)	Dichotic listening task	LP = C < HP	More	Start
DeWall et al. (2011)	2	Not enacted role assignment (HP, LP, C)	Dichotic listening task	LP = C < HP	More	Start
Guinote (2007b)	3	Not enacted role assignment (HP, LP)	Problem-solving task (Drawing a geometric figure without retracing any lines and without lifting the pencil from the paper)	Persistence: HP > LP No. of attempts: HP > LP	More	Start
Guinote (2007c)	1	Episodic priming (HP, LP)	Framed-line test: inhibition of peripheral information	HP > LP	More	Start
Slabu et al. (2013)		Decision: impact of opinion (HP, C)	Spatial cueing task: response time on invalid trials	Short SOA condition: HP < C	More	Start
Smith et al. (2008)	2	Conceptual priming (HP, LP, C)	Stroop task: error rate	HP = C < LP	More	Start
Smith et al. (2008)	4	Conceptual priming (HP, LP, C)	Stroop task: error rate	HP = C < LP	More	Start
Joshi & Fast (2013)	2	Episodic priming (HP, C)	Semi-hypothetical monetary temporal discounting	HP < C	More	Start
Joshi & Fast (2013)	3	Episodic priming (HP, LP, C)	Hypothetical non-monetary temporal discounting	LP = C > HP	More	Start
Joshi & Fast (2013)	1	Not enacted role assignment (HP, LP)	Hypothetical monetary temporal discounting	LP > HP	More	Start

Table 3.2 cont.
Studies on power and self-control classified according to inhibitory and initiatory self-control

Source	Study	Power Manipulation	Dependent Measure (self-control operationalization)	Main Result	HP = more self-control?	Functional component
Guinote (2007a)	1	Not enacted role assignment (HP, LP)	Thought suppression task	HP > LP	Less	Stop
Guinote (2007b)	1	Episodic priming (HP, LP)	Action planning in hypothetical situations: time and information needed	LP > HP	Less	Stop
Smith et al. (2008)	3	Episodic priming (HP, LP, C)	Tower of Hanoi task: error rate	HP = C < LP	More	Stop
Anderson & Galinsky (2006)	4	Episodic priming (HP, LP)	Willingness to engage in unprotected sex	HP > LP	Less	Stop
Guinote (2007b)	2	Episodic priming (HP, LP)	Time needed to initiate goal-directed action	HP < LP	Less	Stop
Lammers et al. (2010)	1	Episodic priming (HP, LP)	Actual cheating behavior	HP > LP	Less	Stop
Scholl & Sassenberg (2015)	2	Episodic priming (HP, LP)	Number of prefactual thoughts	HP < LP	Less	Stop
Scholl & Sassenberg (2015)	1	Not enacted role assignment (HP, LP)	Number of prefactual thoughts	HP < LP	Less	Stop
Scholl & Sassenberg (2014b)	1	Not enacted role assignment (HP, LP)	Time spent on thinking before starting to type an email	LP > HP	Less	Stop

Note. HP = high power condition; LP = low power condition; C = control condition.

Table 4.1
Correlations between selected trait measures and measured experimental variables

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
2	.30**																
3	.31**	.12															
4	.01	-.07	.19^{+}														
5	.04	-.02	.15	.49***													
6	.16	-.03	.16	.37***	.10												
7	-.08	-.03	-.13	-.04	-.30**	-.19^{+}											
8	-.03	.05	-.26*	.01	-.15	-.16	.83***										
9	.16	.03	.09	.06	.06	.13	-.02	-.02									
10	-.17	-.01	-.10	-.13	-.05	-.15	-.08	-.12	-.04								
11	-.09	-.14	.04	.04	-.02	-.01	.23^{+}	.12	-.30*	-.18							
12	-.07	-.15	-.14	.21^{+}	.06	.04	.23^{+}	.17	-.03	-.39**	.12						
13	.16	-.12	-.15	-.12	-.15	.14	-.19	-.16	-.05	-.16	-.27	.10					
14	.04	-.10	-.03	-.01	-.17	.05	-.07	-.08	-.07	-.01	.12	-.20^{+}	.02				
15	-.12	.12	.08	-.01	.06	-.08	.15	.08	.06	.00	.23^{+}	.04	-.77***	-.12			
16	.09	.18	.18	-.03	.13	-.06	-.14	-.14	.16	.01	-.11	.10	.04	-.70***	.10		
17	.17	-.11	.07	-.09	.00	.26*	-.22^{+}	-.17	.27*	.03	-.41***	.00	.12	.10	-.08	-.19	
18	.20	.13	-.04	-.33**	-.14	.00	.00	-.06	.07	.09	-.03	-.11	.01	-.08	.20	.14	.06

Note. 1 = trait power; 2 = situational power in round 1; 3 = situational power in round 2; 4 = DOSPERT all scales; 5 = DOSPERT subscale finance, 6 = 1-item risk measure; 7 = number of safe choices in round 1; 8 = number of safe choices in round 2; 9 = profile correlation thoughts in round 1; 10 = profile correlation thoughts in round 2; 11 = absolute difference thoughts in round 1; 12 = absolute difference thoughts in round 2; 13 = profile correlation feelings in round 1; 14 = profile correlation feelings in round 2; 15 = absolute difference feelings in round 1; 16 = absolute difference feelings in round 2; 17 = recall accuracy in round 1; 18 = recall accuracy in round 2
$^{+}$ $p < .10$. * $p < .05$. ** $p < .01$. *** $p < .001$.

Table 4.2
Means and standard deviations in experimental conditions

	Sample Size[1]	Possible Range	High power first (power loss) condition				Low power first (power gain) condition			
			High power		Low power		High power		Low power	
			M	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
Psychological power (Manipulation Check)	78	1, 7	3.90_{a}	1.06	3.87_{a}	1.11	4.98_{b}	0.74	3.59_{a}	0.92
Sense of responsibility (Manipulation Check)	78	1, 7	5.19_{a}	1.20	4.00_{b}	1.47	5.46_{a}	0.93	4.24_{b}	1.68
Number of safe choices	78	0, 9	5.41_{a}	2.43	5.62_{a}	2.41	5.12_{a}	2.39	5.04_{a}	2.41
Accuracy (Thoughts) – Absolute discrepancy	74	0, 30	6.35_{ab}	3.43	5.65_{ab}	2.96	5.81_{ab}	2.26	6.78_{b}	3.53
Accuracy (Feelings) – Absolute discrepancy	74	0, 114	26.62_{a}	6.79	23.89_{ab}	7.34	24.46_{ab}	7.36	25.27_{ab}	5.12
Accuracy (Thoughts) – Profile correlation	52	-1.0, +1.0	$.08_{a}$	0.54	$.40_{b}$	0.46	$.19_{ab}$	0.57	$.16_{a}$	0.52
Accuracy (Feelings) – Profile correlation	67	-1.0, +1.0	$.47_{a}$	0.26	$.58_{b}$	0.31	$.53_{ab}$	0.25	$.53_{ab}$	0.21
Recall Accuracy – Profile correlation	72	-1.0, +1.0	$-.03_{a}$	0.40	$.19_{b}$	0.48	$.11_{ab}$	0.38	$.18_{ab}$	0.43

Note. Means in the same row that do not share the same subscript differ at $p < .10$ in independent and paired t-Tests. [1]The deviating sample sizes for the accuracy measures stem from the facts that (1) the confederate did not provide ratings of his interaction partner but was rated by his interaction partner, and (2) it was not possible to calculate profile correlations if the standard deviations of item sets concerning thoughts and feelings was zero.

Table 4.3
Results for mixed ANOVAs with the five accuracy measures as dependent variables

	Thoughts								Feelings								Recall			
	Profile correlation				Absolute difference				Profile correlation				Absolute difference				Profile correlation			
Effects	*df*	*F*	*p*	η_p^2	*df*	*F*	*p*	η_p^2	*df*	*F*	*p*	η_p^2	*df*	*F*	*p*	η_p^2	*df*	*F*	*p*	η_p^2
Power	1,48	0.41	.53	.008	1,63	0.21	.65	.003	1,63	1.42	.24	.020	1,63	1.28	.26	.020	1,68	5.47*	.02	.070
Order	1,48	0.25	.62	.005	1,63	0.01	.92	<.001	1,63	0.14	.71	.002	1,63	0.13	.71	.002	1,68	0.17	.68	.002
Responsibility	1,48	0.04	.85	<.001	1,63	0.41	.53	.006	1,63	0.64	.43	.010	1,63	0.05	.83	<.001	1,68	0.93	.34	.010
Order x Power	1,48	3.18$^+$	.08	.060	1,63	4.18$^+$	.05	.060	1,63	4.41*	.04	.070	1,63	6.52*	.01	.090	1,68	2.90$^+$	.09	.040
Order x Responsibility	1,48	0.01	.92	<.001	1,63	0.14	.71	.002	1,63	0.01	.92	<.001	1,63	0.05	.83	<.001	1,68	0.50	.48	.007
Power x Responsibility	1,48	0.11	.74	.002	1,63	0.40	.53	.006	1,63	3.41$^+$	.07	.050	1,63	1.35	.25	.020	1,68	1.63	.21	.020
Power x Order x Responsibility	1,48	3.55$^+$	.07	.070	1,63	0.02	.90	<.001	1,63	0.83	.37	.010	1,63	0.22	.64	.003	1,68	0.64	.43	.009

$^+p < .10$. $^*p < .05$.

Table 5.5
Descriptive statistics and correlations among study variables

Variables	*M*	*SD*	*n*	1	2	3	4	5	6	7
1. Low situational power	0.35	0.48	1139							
2. High situational power	0.24	0.43	1139	-.41***						
3. Sense of power	2.98	1.10	1126	-.52***	.41***					
4. Personal power	3.56	0.89	1126	-.09**	.09**	.19***				
5. Sense of responsibility	3.27	1.03	1126	-.28***	.29***	.57***	.08**			
6. Start SC	75.44	24.96	1121	.02	-.02	-.04	.10**	.02		
7. Stop SC	73.47	27.37	1118	.00	-.02	.03	.02	.00	.19***	
8. Interpersonal SC	67.61	29.36	1120	-.03	-.01	.02	.00	.00	.09**	.15***

Note. SC = Self-control. Correlations were calculated using person-mean centered variables (exception: situational power). Sense of power, personal power and sense of responsibility were measured on scales ranging from 1 to 5, whereas the three self-control dimensions were measured on scales ranging from 0 to 100.
*** $p < .001$. ** $p < .01$. $^{+}$ $p < .10$.

Table 5.7
Results of multilevel analyses testing propositions 2 – 3b and 8 – 10

Criterion	Sense of power	Personal power	Sense of responsibility	Start self-control	Stop self-control	Interpersonal self-control
			Fixed effects			
Parameter						
Intercept	3.13*** (0.08)	3.58*** (0.05)	3.26*** (0.04)	74.90*** (1.65)	72.91*** (1.72)	68.59*** (2.07)
Level 1						
(Measurement occasions)						
Low situational power	-1.00*** (0.07)	---	---	---	---	---
High situational power	0.99*** (0.10)	---	---	---	---	---
Sense of power	---	0.14*** (0.03)	0.59*** (0.04)	-1.26* (0.62)	0.67 (0.71)	0.55 (0.73)
Personal power	---	---	---	2.99*** (0.88)	0.48 (1.00)	---
Sense of responsibility	---	---	---	---	---	-0.39 (0.75)
Level 2 (Individuals)						
Low situational power	-0.13 (0.17)	---	---	---	---	---
High situational power	-0.02 (0.16)	---	---	---	---	---
Sense of power	---	0.31*** (0.07)	0.61*** (0.05)	-3.29 (2.55)	-5.83* (2.67)	-0.26 (4.03)
Personal power	---	---	---	7.33* (2.91)	9.92** (3.04)	---
Sense of responsibility	---	---	---	---	---	-4.01 (4.41)
			Random effects			
Variance						
Intercept	0.10 (0.32)	0.28 (0.52)	0.12 (0.34)	293.08 (17.12)	311.02 (17.64)	493.73 (22.22)
Low situational power	0.19 (0.43)	---	---	---	---	---
High situational power	0.43 (0.66)	---	---	---	---	---
Sense of power	---	0.03 (0.16)	0.08 (0.29)	/	/	/
Personal power	---	---	---	/	/	---
Sense of responsibility	---	---	---	---	---	/
Residual	0.39 (0.63)	0.41 (0.64)	0.50 (0.71)	329.68 (18.16)	422.42 (20.55)	319.62 (17.88)
R^2	62.0%	37.6%	46.2%	33.4%	30.3%	50.7%

Note. --- = Predictor was not part of the model because it was not part of the hypotheses. / = Predictor was not part of the model because a comparison of BIC indicated that is was not adequate to add random slopes. For fixed effects standard errors are in parentheses. For random effects standard deviations are in parentheses. (Pseudo-)R^2 was calculated comparing the absolute null model without any predictor to the model presented in this table.
*** $p < .001$. ** $p < .01$. * $p < .05$.